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CLINICAL LECTURES.

THE RADICAL CURE OF NON CONGENITAL INGUINAL HERNIA.

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PARIS, FRANCE.

Gentlemen:—I am about to operate before you upon this patient, a man 55 years old, who is suffering from an inguinal hernia in the left groin. This hernia is of about ten years standing, it developed slowly and progressively. Actually its weight, if isolated, would be about two pounds. It can be easily and completely reduced. After its reduction has been effected, one is struck with the considerable dilatation of the inguinal canal. In truth, the canal has entirely disappeared, and nothing is left but a large orifice which is a fusion of the internal and external orifices, without any intervening passage. On examination of the right side, we see that here the inguinal canal is also greatly dilated, and at its opening there are also slight evidences of hernia.

This extreme dilatation of the orifices of both inguinal canals is, I believe, the reason our patient has suffered so much from hernia. In this case there are actually no traces of any adhesions, and the hernia is completely and easily reducible. Before proceeding to undertake the radical cure of this case, which naturally involves an operation, we must have decided indications which will fully justify our taking such a step in a patient of the comparatively advanced age of fifty-five. Our indications, which fully warrant the performance of the operation are the following: Although the reduction of the hernia is easily accomplished, its retention in place is most difficult and troublesome owing to the size of the inguinal orifice. The patient has resorted to many styles of trusses, but all are either incapable of retaining the hernia in place or else cannot be tolerated by the patient. The hernia invariably, upon the slightest exertion on the part of the patient, will fall above or below the pad of the truss. The various experiments result in the production of intestinal irritation, or else inflammation, sufficiently violent and menacing to justify the performance of the operation for the radical cure of the hernia. Thanks to the size of the inguinal ring there has been no strangulation in the case, but it is very likely that this complication might occur at any time, necessitating an immediate surgical interference under circumstances much less favorable, and far more dangerous than those existing at present.

This question of the justifiability and value of the radical cure for hernia, has, as you know, excited a vast amount of controversy among surgeons. After the attempts made by surgeons of the last century to perfect this operation, the results they have obtained caused the procedure to fall into disrepute in the beginning of this century. This disfavor was entirely justifiable, since it was then impossible to prevent accidental traumatic infection, but with our present knowledge of antisepsis the operation is rendered quite inoffensive, although very possibly capable of considerable improvement. All the same, as had been said, the subject is still one of controversy among surgeons: First, in regard to the adequate indications for its performance; second, in regard to the efficacy of the operation itself.

Concerning the indications for the radical cure, one is to-day indisputable, that is for strangulated hernia. In the operations for strangulated hernia the "radical cure" is embraced in the final step of the operation. The intestine having been reduced, it is necessary to resect the hernial sac. Without in any way adding to the dangers of the first step in the operation for strangulated hernia, its conclusion by the resection of the sac will only tend to lessen the dangers of an ultimate relapse. Finally, in case of a relapse

*Delivered at the *Hôpital St. Louis*, Paris; reported from *Le Bulletin Médical*.

the hernia will be more easily held in place and better tolerated.

The indications for the operation in cases of irreducible herniæ are to my mind equally forcible. A hernia of this kind is a source of constant pain and perpetual menace to the patient. Trusses are not tolerated, and even specially designed apparatus gives but a poor sort of satisfaction. The radical cure will at least transform the irreducible hernia into a reducible one, which can be retained by a very simple truss, and this alone is surely a decided gain.

Regarding the operative difficulties in such cases, I would say that the irreducibility of these herniæ may be due to either of two different causes: There may be adhesions of the intestine to the hernial sac, or else the presence of the epiploon or omentum in the sac may constitute the obstacle hindering the reduction, either by reason of adhesions or else by reason of its volume. It is not rare, in fact, for the omentum to become tumefied or swollen, and to become chronically inflamed while incarcerated in the hernial sac, forming a sort of stopper or cork in the inguinal ring which will prevent reduction. The operative technique, as well as other difficulties, will vary with indications of this or that case.

A third order of indications, and one which has been freely discussed and disputed, is, as in the case of our patient, a difficulty in keeping the hernia in place. In order to be justified in operating upon this class of cases, it is evident that the reality of this difficulty must be fully established, and that the various styles of suitable trusses have been conscientiously tried. If this has been done, we are surely justified in not leaving the patient menaced by constant suffering, and the possible accidents of rupture or strangulation.

But before deciding upon a surgical interference the general condition of the patient should be most carefully inquired into. As I have already said, the age of our patient (fifty-five years) has caused some hesitancy on my part, and I would not have concluded to operate had the patient not been robust, active, and well kept. In case, however, the patient were feeble, poorly nourished, or had any suspicion of pulmonary trouble, I would refuse to operate. Even in a case of irreducible hernia, were the patient's condition bad, we would be justified in confining the operation to a simple kelotomy without attempting the radical cure. In patients of advanced age, simple kelotomy constitutes a grave procedure, and any prolongation of the operation should be rejected.

These constitute the indications and contra-indications for the radical cure of hernia. What at present constitutes the dangers and the results of the procedure?

The operative dangers, in a suitable case, and when the operation is conducted with antiseptic precautions, may be regarded as nil. I, for my part, have performed the operation nearly fifty times, and my statistics, as also those of other antiseptic surgeons, show no death. I only once had an accident, which very fortunately was not serious, but which I will refer to in order to show you the importance of maintaining strict asepsis.

The patient in question was operated upon by me in the Hôtel-Dieu, and an abscess formed on the fifth day at the neck of the sac. The catgut employed as the ligature had evidently been improperly sterilized and had acted as the exciting cause for this infection. It would, you will therefore see, gentlemen, be useless for you to attempt the operation for the radical cure of hernia, did you not fully comply with all the requirements of strict asepsis.

Regarding the therapeutic results which one may hope for, it will first be necessary to distinguish two different operative conditions.

Among herniæ, one variety, as you know, is produced by force, strain or rupture, and occurs in patients, with strong abdominal walls, but whose vocation in life compels repeated and violent exertions, these would include butchers and bakers for instance. These may be classed as accidental herniæ. The second variety of herniæ comprises those resulting from feebleness, or constitutional herniæ. These are the herniæ met with among aged and debilitated subjects. There exists but slight resistance of the abdominal wall. When speaking of the ultimate results of this operation for radical cure, I also add to this last group congenital herniæ, caused by malformation, a veritable defect in the abdominal wall.

The herniæ of force promise the best results after the operation for radical cure. The recovery is complete, and it is lasting. Two cases of this kind come to my mind, both of which I operated upon over two years ago. In both the cicatrices remained firm and solid, and in spite of the most fatiguing and continued labor on the part of the subjects, there has been no evidence of recurrence of the herniæ. I still advise them to wear a truss, but more as a precaution than a necessity. In one of these cases the hernia was very voluminous, and I was only able to reduce it after the most difficult and laborious operation of the kind that it has yet been my

lot to perform. In the other subject, the continued strain which his occupation has compelled him to work under since the operation has been sufficiently violent to give rise to a hernia on the side not operated upon, while the old hernia has given no evidence of reappearing. These results are very satisfactory.

The herniæ of feebleness, on the contrary, will only result in moderate success when treated by the radical cure. In these cases one may be able to suppress the hernia, but its cause itself, the predisposition caused by the feebleness of the abdominal walls, remains after the operation has been apparently sucessfully completed.

The least successful case that I ever operated upon was that of a child four years old. The hernia was congenital, and returned four months after the operation, as large at it had been at first. But the subject was very feeble, and subsequently died of tuberculosis.

The technique of the operation for the radical cure is simple enough. While maintaining strict asepsis, I do not insist upon all the technical minutiæ, but am satisfied with knowing that the patient's parts to be operated upon, instruments, operator, and assistants are clean. The incision is made cautiously, line by line, until the sac is reached. The laying bare of the sac will be found especially difficult if at the moment of incision the hernia slips back into the abdomen. To avoid this make the patient cough so as to bring the hernia down before administering ether, and avoid the slightest attempt at a reduction.

The sac, when opened to the view of the operation is incised, and we may be able to see the contents of the hernia, whether it be a loop of intestine or a portion of the omentum.

The intestine, if it is non-adherent, can be reduced with the greatest ease, and even if there exist slight adhesions of the intestine to the sac, these may also be easily liberated and the intestine reduced. But the case is quite different if there are old firm adhesions. In such an event the greatest care must be taken in liberating the intestines from the sac, and especially the peritoneal portion of the omentum if involved. These firm intestinal adhesions are, very fortunately, rarely met with. I can only remember one such in all my experience.

The omentum is nearly always adherent, but its liberation is fortunately far less dangerous than that of the intestines. But when liberated it should be resected and not returned to the abdominal cavity. It is advisable to resect as much of the epiploon or omentum as possible, and to accomplish this the omentum should be pulled gently forward, exposing a portion of the intra-abdominal part. If too little of the omentum is excised, the remaining part will not be well drawn up into the abdomen and this may cause an ultimate return of the trouble. If you reduce the omentum and do not excise any portion of it, the chances of its pressure upon the opening of the inguinal orifice causing a relapse, are very large.

After the omentum has been resected the intestines are reduced, a sponge placed in the inguinal orifice, and the resection of the sac is undertaken. This should also be done as high up as possible.

Nothing further remains to be done but to insert a row of superficial sutures in the skin, and one or two deep ones. I would caution you not to forget the use of a drainage tube, as it is of considerable advantage in obviating any danger of septic infection. The wound is then dressed with iodoform and a bandage applied.

The patient before the class was then etherized and the operation begun, and conducted as described. There were no intestinal adhesions or other complications. The patient made good recovery from the effects of the operation, which lasted but fifteen minutes.

HEMIPLEGIA.

By PROF. CHARLES CARY, M. D.

BUFFALO GENERAL HOSPITAL—MEDICAL CLINIC.

This patient, aged twenty-eight, was brought into the hospital six days ago by the Fitch ambulance, with the history of having been stricken with paralysis on the street. He was found aphasic, with right hemiplegia and facial paralysis. He could feel a pin prick in this side. He could swallow with difficulty, and the tongue could not be protruded. The evacuations of urine and fæces were involuntary.

So far as the previous history is concerned, we can learn nothing. We must determine the anatomical lesions, and the condition resulting, entirely from our own observations. Although the patient can tell us nothing of his condition, you will notice as we go through the examination that he is more or less conscious and capable of understanding us to some extent. His mind has cleared up a little since he first entered, when he was inclined to stupor, but not to such a degree as to constitute coma. When I hold his

nose to force him to open his mouth, you will observe that the movement of the lips is only upon the left side. He shuts his eyes, the left completely, the right only partially; the superior branches of the facial nerve being seldom affected to the degree that the lower ones are. When I ask him to protrude the tongue, he evidently understands me and makes an effort to do it which is partially successful, but the tongue is tilted to the paralyzed side. This is due to paralysis of the hypoglossal nerve. There is no motor nor sensory disturbance of the left side.

The patient's hands are hard and he is evidently a man used to manual labor, but he has not the appearance of having been a drinking man, and he has neither the age nor the build which we usually associate with apoplexy. The right hand is a trifle more puffy than the left, and it has an inactive look. On asking him to hold up his right arm, he tries to respond, and assures me by his expression that he can not do it. There is at present a scaly eruption over the right forearm, and a day or two ago it had quite a papular appearance. This is not at all uncommon in cerebral hæmorrhage, and is due to a trophic disturbance. When I first saw him there was profuse sweating of the entire right side, and attended vaso-motor dilatation. There is no question but that his sensations, if not absolutely normal, are but slightly disturbed in the right side. The mild œdema seen here, and which is often found shortly after paralysis, can be explained in this way. The venous return from the extremities is largely due to muscular contraction. If you should hold your arm in the same position for forty-eight hours the lines would be more or less obliterated on account of a very slight œdema a dropsical effusion all through the muscles, and particularly in the subcutaneous cellular tissue. This is due, therefore, in paralysis, not entirely to vaso-motor disturbances, but perhaps even more to the inactivity of the muscles.

When the patient first came in, tickling his right foot would cause it to be removed. On tickling the right foot now, the response is more in the left foot. The former movement was purely reflex—an involuntary, spinal action of a protective nature. The present reflex has more the nature of a conscious effort to drive away the irritating cause. When there is no sensation, tickling of the foot will usually induce quite complete movement of the leg. There is now a certain resistance as I move the paralyzed leg; and this is not uncommon in recent paralysis. It is of the same reflex nature as the attempted withdrawal from the finger on tickling the foot. The patellar reflex is usually rather exaggerated after paralysis, but here it is apparently much reduced. The reflex, however, is equally slight on the normal side.

When the patient was first brought in, his breathing and heart-action were very slow, the slowness being undoubtedly due to the shock he had received at the time of the vascular rupture. The first sound of the heart is normal, sharp possibly, and a trifle prolonged, but without murmur. With the second sound, there is a dragging murmur, not a bellows murmur in any sense, but almost such a sound as you would get from dragging the foot over the floor. I do not know but that it is an uncertain and irregular closing of the aortic valves. It seems a little too distant to be a friction sound. I am not inclined to consider it a regurgitant murmur, for if the patient had had a long continued aortic insufficiency, he would have quite a degree of cardiac hypertrophy or dilation which is not indicated by the point of the apex impulse nor by the area of cardiac dullness. Acknowledging at the outset that the determination of this man's cardiac lesion has not been effected, I wish to wait before stating how far the cerebral lesion depends upon that in the heart. I am inclined to think that he has had a minute aneurism of one of the cerebral arteries, and I take this view because this is the most common cause of cerebral hæmorrhage. I imagine that during some temporary excitement or exertion the force of the heart ruptured this aneurism and poured out a mass of blood which caused the paralysis.

As to the location of the rupture, all evidence places it upon the left side of the brain, either in the cortex or more probably in the neighborhood of the lateral ventricle, that being the locality where the vessels are most numerous and where rupture most commonly occurs. The amount of blood poured out, we cannot determine; sometimes a small clot will produce all the symptoms which we note here, and again a large clot may occasion comparatively slight results.

As to the outcome, I should expect this man to improve and rather rapidly, but I question very much whether he will recover completely for the reason that the arterial disease, the arteritis, which is the probable cause of this aneurism which, we have supposed, has ruptured, makes it probable that under similar circumstances of increased excitement or exertion and consequently heightened arterial tension, and particularly as

age advances, rupture may again occur, not necessarily in the same place, but in some other part of the brain.

The patient has presented no symptoms requiring immediate relief. In any case of paralysis, attention must be given to the condition of the bladder and the bowels so as not to allow retention of urine and fæces. This man has incontinence and not retention. If he had a high bounding pulse with great hypertrophy of the heart, I should have thought blood-letting of service. This, however, is rarely resorted to of late years. Five or six years ago, I bled a man and to the best of my knowledge the operation has not been done in this hospital since. This case I saw very early, the arterial tension was high and I believed the hæmorrhage in the brain to be still in progress. Elevation of the head, cold applications to the head and stimulation of the bowels to draw blood from the upper portion of the trunk and head, would in most cases be about as effective as venesection. I prefer saline cathartics, but it is the custom with many to use a more irritating cathartic, such as croton oil. For the time being, this patient is simply being watched and he is recovering, as such patients generally do unless they die from the first shock, which is not an uncommon thing. In the course of the next five or six days, we shall institute a plan of treatment which will, perhaps, render some relief. I say *perhaps* for the action of our remedies, especially in cases of hemiplegia, is not always satisfactory. I shall put him upon potassium iodide for several reasons. In the first place, because the alkali which it contains is rather good to reduce arterial tension; the iodine promotes absorption as much as any agent that can be used for that purpose, and the blood clot in this patient's brain can be removed only in this way. Again, the cause of miliary aneurism is very commonly endarteritis of syphilitic origin. It would be difficult for me to suggest why a man of this age, with such apparently good health, should have cerebral aneurism even of small size except as the result of syphilis. Do not think of miliary aneurisms as being as big as beans or peas even. They are extremely minute dilatations of small arterioles. This man has been carefully examined for syphilitic markings without finding any. Miliary aneurisms, however, are often due to the rheumatic taint and potassium iodide is as good for a suppressed form of rheumatism as any remedy that can be used. After a few days, in order to prevent degeneration of the muscles, we will undertake massage and passive motion of all the joints, and especially making complete extension, for even now I notice that the flexor muscles are beginning to gain the ascendency over the extensors, and if a contracture once occurs it can never be fully overcome.

COMMUNICATIONS.

A DISCUSSION ON PHAGOCYTOSIS AND IMMUNITY *

At the Pathological Society of London, March 1st, 1892.

The discussion on Phagocytosis and Immunity, which was adjourned from the last meeting, was resumed (Sir GEORGE MURRAY HUMPHRY, F.R.S., President in the chair) by

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Dr. Bristowe approached the subject from a clinical point of view; he showed that the resistance of the body to the invasion of infective diseases was provided for in various ways: (*a*) mechanical, as the opposition of the skin and mucous membranes; (*b*) by inflammation with subsequent encapsulization or suppuration; (*c*) by active warfare or phagocytosis; (*d*) by unfitness of soil, etc. With regard to (*c*), he agreed with Dr. Burdon Sanderson's position as stated at the last meeting. Should the infective principle succeed in getting past these first barriers, they would enter the system along the blood vessels, or more commonly along the lymphatics as far as the first group of glands. He thought that cure occurred by the infective principle getting beaten at the mucous membrane or at the lymphatic glands; but these modes of spontaneous cure would not include the cure of the exanthemata after their generalization; and it was in these cases especially that immunity most frequently arose.

He then contrasted various types of infectious diseases, taking tubercle, diphtheria, and typhoid and small-pox as types of the different varieties. Local skin tubercle or lupus, instead of producing general tuberculosis, seemed to prevent it. In diphtheria and typhoid the chief symptoms were associated with the local lesions rather than with the generalized poison, which as a rule produced its effects after the subsidence of the local lesions. With small-pox and the other exanthemata the conditions were wholly different. Taking

*Continued from last issue of the REPORTER.

inoculated small-pox as an example, the poison appeared to be developed for nine days, with formation of the pustule and enlargement of neighboring glands; then the generalized condition set in, with eruption on the third day. Immunity seemed to be acquired in the earlier stage of the disease, as it was remarkable that whilst the pocks were developing the poison from them was not absorbed by the body. Probably the pocks were really cut off from communication with the rest of the body by phagocytosis and by the inflammation which surrounded them. By vaccination the generalized disease that was produced and which gave immunity was comparable to the invasion period of small-pox, but he could not say how the immunity was caused. He suggested that it might be possible to avoid the local ill effects of vaccination by injecting the lymph deeply into the tissues or into the glands.

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PART I.—RELATING TO IMMUNITY.

Before discussing the probability of one or other doctrine, it is well to have the facts clearly before our minds. The facts, then—to take the simplest case—are briefly these: that in non-protected animals virulent bacteria inoculated subcutaneously produce little or no local change, grow unhindered, and cause general infection; while in protected animals inoculation of the same bacteria is followed by a local leucocytosis at the point of injection, a certain degree of inflammation is set up, and general infection is prevented.

In virtue of some change in the body, that which was formerly capable of setting up a general disease can now only set up a local affection. What is the nature of this change? On the one hand, we have the bacteria with their powers of forming poisons undoubtedly capable in many instances of affecting the organism most injuriously. These poisons are of two kinds: (1) easily diffusible substances of alkaloidal nature—toxines—whose mode of action on the organism is analogous to that of the vegetable alkaloids; and (2) less diffusible substances of proteid nature, whose nature is still very imperfectly known. So far as is known, however, their action resembles more that of ferments than of ordinary chemical poisons, inasmuch as it is lost on heating to a temperature of 50° or 60° C., at which the active properties of ferments are generally destroyed. Both these classes of poisons stand in the same relation to the bacteria, in so far as they are the special products of their activity, in the same way that nitrogenous products, such as urea, or active proteid substances, such as ptyalin or pepsin, are the products of the activity of cells of higher organisms.

In addition to these two groups of metabolic products formed by bacteria, there is another class, differing both in origin and action from the foregoing. These are also proteid bodies, having, so far as is known, the character of albumoses. These are substances—first described by Nencki in 1880—derived from the bodies of the bacteria themselves, so-called "proteins," for a knowledge of whose action we are indebted, perhaps, most of all to Buchner. They resist in a remarkable way the action of heat, withstanding when in a moist condition even boiling temperatures. Their most remarkable property, however, is their chemiotactic power, the power of attracting wandering cells, and inducing, therefore, a greater or less degree of leucocytosis wherever they happen to be present. Their action in this respect I hope to illustrate to you later on.

It is to be noted as a further difference between these two classes of bacterial products, the toxines and toxalbumens, on the one hand, and the proteins on the other, that the former are only formed, and are formed most abundantly when the bacteria are active, the latter only, and most abundantly, when the bacteria are dying.

A further difference—an important physiological one—may be noted, namely, that while the former are responsible for the general symptoms of disease, the action of the latter is, in the first instance at least, mainly a local one. Without being entirely confined to the part where they are formed, the most important action of these proteins, and the one which lends them their chief interest, is the chemiotactic power above mentioned.

When we turn now from the invading bacteria to the organism thus attacked, we find that the means it has at hand to defend itself are of a two-fold character. On the one hand, there are the cells—especially those free lances of the body, the wandering leucocytes—having the power of seizing upon everything, animate or inanimate, possessing a less power of resistance than themselves, and, in virtue of this power, doing undoubtedly much useful work for the organism both in health and disease. The theory of phagocytosis, so beautifully and skilfully elaborated by Metschnikoff, in the first instance, assumes, on the basis of many observations whose accuracy cannot be seriously

called in question, that this power is exercised in a special degree by the cells in defending or protecting the organism against bacteria.

This is admittedly, however, not the only weapon possessed by the body in its defense. Apart altogether from the action of living cells, the plasma and serum of the blood is now known, through a series of equally beautiful researches, to possess, under certain circumstances, antibiotic properties when withdrawn from the body, bacteria being killed after a short exposure to their action.

It has been shown, moreover, for certain bacteria (for example, vibrio Metschnikovi, by Behring, and Nissen) that the power which the body possesses of resisting their action is proportional to the antibiotic properties of its blood serum, the serum of animals rendered immune having strong bacteria-killing power, while that of the ordinary animal favors the growth of the bacteria. This then is the basis of the view opposed to the phagocyte doctrine, of the so-called humoral doctrine. It is assumed by the upholders of this view that the protecting power possessed by the body is really due to these antibiotic properties of blood serum to the action of the fluids of the body rather than of the cells.

Unfortunately for this view which, in presence of such facts as those above referred to, seems a reasonable, not to say a seductive, one, it is admitted that this relation between degree of protection possessed by the body and the antibiotic properties of its blood serum is not only not a constant, but that in certain cases it is completely reversed, and that the serum of an animal very sensitive to the attack of certain bacteria, when these are injected into the body, may be nevertheless extremely bactericidal in its action on these bacteria when tested outside the body. Thus it has been shown by Roux and Metschnikoff that the serum of young rats sensitive to anthrax has a bactericidal action on anthrax bacilli outside the body; and by Lubarsch that the extravascular blood of a rabbit can kill millions of anthrax bacilli, while a few of these injected into the blood suffice to kill the animal.

It is clear, then, that some caution is necessary in deducing from the action of blood plasma and blood serum after it is withdrawn from the body what its action within the body is likely to be. So far as the evidence at present goes, there is no established relation between the two.

As regards the nature of the substance or substances in plasma or serum to which this bactericidal action is due, little is known further than that they are proteids. In one case the substance was found to have the characters of globulin (Hankin); but with this exception—and the observation is an isolated one—but little progress has yet been made in identifying these substances with any special class of proteids. The most recent work renders it to my mind exceedingly probable that it is not to any one definitely characterizable proteid substance that this bactericidal property of serum is due, but rather to some special association, of an altogether more complex character, of one or more proteids.

The recent work of Dr. Wright, on which I trust we may hear something from him in this discussion, appears to me to have a special interest in this relation, and a very direct bearing on this point.

He has shown that the "tissue fibrinogen" to which Wooldridge ascribed such an important *rôle* in coagulation phenomena, and which he found also to be capable of conferring a certain degree of protection against anthrax, is, in all probability, a double proteid body, one of albumose nature and the other a nuclein substance.

PART II.—INTERPRETATION OF THESE FACTS.

So much, then, for the facts on which, as Professor Sanderson has said, we are for the most part agreed, and so much incidentally for certain of the difficulties that arise in interpreting aright their significance. If we now consider the application of these facts to the phenomena of acquired immunity, as above described, we find that the divergences of opinion which exist amongst bacteriologists relate more to the order of occurrence and relative importance of the events than to the events themselves.

The two chief events are that a short time after the inoculation of virulent bacteria into an immune animal a large number of leucocytes are found at the seat of inoculation, and a large number of the bacteria are found lying enclosed within these leucocytes and other adjacent cells.

According to the doctrine of phagocytosis, the leucocytes have been attracted thitherwards by the chemiotactic action of certain of the products of bacterial action; that they then act as phagocytes, and engulf the bacteria. It is not denied that in "reacting" to the presence of these products they may likewise produce substances capable of acting on the bacteria injuriously without the latter being taken up. It is nevertheless maintained that the intracellular method is the chief method employed in injuring the bac-

teria. The protein substances given off by the dying bacteria exert further chemiotactic action, and attract more leucocytes, till finally, at the seat of attack, is mobilized a regular army of agents capable of resisting successfully all further invasion.

The opponents of this view maintain that the first event is not an attraction of leucocytes, but a death of certain of the bacteria brought about through the agency of bactericidal substances present in the serum, and that the leucocytosis is secondarily induced by the proteins set free from the injured bacteria. The subsequent inclusion of the bacteria within these cells is an event of altogether secondary importance. The cells in taking up the bacteria exercise merely their ordinary scavenging function, and not any special phagocytic function.

The question at issue between the two opposing schools of "phagocytists" and "humoralists," as will be seen, is thus one that lends itself readily to active discussion and controversy, for the differences between them are those of degree rather than of kind.

The phagocytists assert that their opponents attach too much importance to the bactericidal action of blood serum, and too little to the cells; while the latter report that the so-called phagocytic function is a purely scavenging function, and that the chief factor in inducing the leucocytosis is the production of proteins from the already dying bacteria.

In the words of Buchner, the chief exponent of the humoral theory, the essential active processes underlying immunity can only be of chemical nature. According to this theory it is a fatal error to assume that in protected animals the bacteria find themselves precisely the same conditions, before leucocytosis sets in as in non-protected animals. According to the doctrine of phagocytosis, it is the special reaction of cell protoplasm that determines the issue, the action of chemical products being of secondary importance. A certain antagonism there undoubtedly therefore is between the two views; but the doctrines are not in my opinion so opposed to each other as to be mutually destructive.

I gather from the course the discussion has so far taken that some misapprehension seems to exist on this point. It is assumed that there is an inherent antagonism between the two views, and that to establish the one it is only necessary to show up the weakness of the other. Acting on this assumption, the speakers so far have confined themselves mainly to pointing out in how far the doctrine of phagocytosis falls short of perfection. The result has been to show that in their opinion it falls very far short indeed of that point; for we have heard that not only is it imperfect, as we all admit, but that it is utterly opposed to general biological principles; much of it untrue; what of it is true is not new; it is illogical, mechanical, in fact altogether unsatisfactory. Having made out this case to their own satisfaction, the speakers quietly assume that they have firmly established the immense superiority of the opposing view.

Their practice appears to be based on one, if report speaks true, not altogether unknown at the bar, which consists in supporting a case itself weak, by exposing the weak points of the one opposed to them. In the present instance it takes the form of abusing the doctrine of phagocytosis, and letting judgment go by default in favor of the "humoral" doctrine. They have unconsciously accepted a brief for the latter doctrine, and have done their best for their client by abusing the opposing one.

Like many other arguments that in the first instance appear very strong those adduced against the correctness of the doctrine of phagocytosis lose in my opinion much of their weight on being carefully looked into. I feel myself freer to express this opinion because I am not at present appearing on behalf of that view, but have accepted a "watching brief" on behalf of a client of my own; an intervening client whose interests and reputation appear to me not unlikely to suffer in the action thus raised against the doctrine of phagocytosis.

Exercising my right, I do not choose to divulge this client's name.

Objections to the Doctrine of Phagocytosis.—The objections raised in this discussion against the doctrine of phagocytosis group themselves into two divisions: (1) General, those which impugn the inherent probability of the doctrine charge it with being mechanical, illogical, opposed to biological principles; and (2) special, those which dispute the accuracy of the data on which it is based.

1. *General Objections.*—With regard to the more general objections, it is extraordinary how much opposition has been called forth by this doctrine, and how much warmth of feeling has been imported into its discussion by the parties on both sides. It is little wonder that outside witnesses of the controversy should conclude that some deep underlying principle must be at stake, and that the two views must be utterly opposed to each other.

What then are the principles involved? On the one hand it is asserted by the upholders of the doctrine of phagocytosis that living cells have the power, under certain circumstances, of taking up living bacterial organisms; of acting on them in such a way as first to kill them and then completely destroy them.

In other words, it is asserted that one form of living protoplasm has the power of taking up within itself another and presumably weaker form and destroying it altogether. If any biological principle is more firmly established than another, it is surely that such a power is possessed by protoplasm. It is maintained, further, that this is the chief way in which the body protects itself against attacks of infectious disease, and that the difference between one who has had an attack of an infectious disease—for example, scarlet fever—and has thereby acquired protection, and one not so protected, is that in the former the individual cells have acquired some additional property, which enables them to resist successfully from the beginning where previously they had only been able to do so after the disease had run a certain course.

As thus put, the doctrine seems to lay itself open to the charges brought against it; that it does not add much to what we previously knew; that it only expresses in terms of the cell what we have long been in the habit of stating equally clearly in terms of the body as a whole; moreover, that it brings no new properties of the cell to light, for that we have long known that the cells had a sort of scavenging function, and were in the habit of taking up, so to speak, whatever they could lay hold of.

Further, as Dr. Klein would maintain, it is not proved that the bacteria find themselves so uncomfortable with the body as is generally supposed; that possibly the cells, so far from enjoying their repast of microorganisms, suffer as often as not from surfeit, and themselves die, victims of their own voracity. In short, the doctrine is in great part not new, true, nor logical.

In reply to these various objections it may be urged with some force that, even if it had only directed our attention from the body as a whole to processes going on in the individual cells of that body, an important step would, in my opinion, have been taken—a step in the direction in which all advance in pathology has been, and is destined to be, made—namely, from the general to the special. The history of pathology is a history of such advance, from the spirits which formerly caused disease, independent of the body, to the body itself; from the body as a whole, with its vapors and humors to the particular organs involved; from the organs to the special cell structures implicated; from these in turn to the nature of the disturbing cause. Further, that while it is not claimed that any new property of cell protoplasm has been discovered, the observations on which the doctrine of phagocytosis is based are an important extension of our knowledge of cell function in an entirely new direction. It is the application of the teachings of cellular pathology to the phenomena of infectious disease and immunity from that disease, and to that extent it can fairly lay claim to have added something new to our knowledge.

As to Dr. Klein's demonstration of the phagocytic process in leprosy and other chronic diseases, and his contention that it can, therefore, have nothing to do with immunity, I venture to submit that that contention is altogether beside the mark, for it is not asserted that phagocytosis and immunity are synonymous terms, and the contention fails altogether to take note of the difference between an acute infectious disease and one running a slow course such as leprosy. The upholders of the doctrine of phagocytosis cannot be held responsible for these differences; they are only concerned, so far as they can, to explain them. And when we endeavor to do this, we find that not only does the above contention fall altogether to the ground as an argument against the relation of phagocytosis to immunity, but it lends, in my opinion, strong support to that view; for chronicity of an infectious disease like leprosy or tuberculosis can only be due to one or other, or both, of two causes —relative weakness and harmlessness of the attacking microbe, that enables it to maintain a precarious existence by not exciting any particular disturbance, or only a local disturbance; or, secondly, presuming the microbe to be fairly strong, great and prolonged resistance carried on on the part of the body for a term of years.

In warfare, next to a complete defeat of the enemy, that which speaks most for the efficiency of a force is its power of carrying on a protracted struggle against superior forces. And so likewise in infectious disease. Next to successful resistance once for all to a disease as in measles or scarlet fever, that which speaks most for the resisting power of the body is chronicity of the disease itself. When, therefore, Dr. Klein points to phagocytes crammed with bacilli in leprosy, he points to facts that, so far from upsetting the doctrine of phagocytosis, lend strong support

to it, for chronicity of an infectious disease I would define as a relative immunity. His argument would have had weight against the phagocyte doctrine if he had been able to point to leprosy or other chronic infectious disease, and demonstrate that no bacilli were to be found in cells, and yet the body had been able to carry on its struggle for many years. As the facts stand, any force his argument has is in favor of the phagocyte doctrine.

And when we come to the question of logic, which position is the more logical—that which ascribes to cell protoplasm and direct cell action, as the doctrine of phagocytosis does, or that which ascribes to certain products of that activity acting at a distance from the cell, as the opponents of that view maintain, the power of combating and ultimately destroying pathogenic micro-organisms, I confess for my own part I have little hesitation in giving my opinion.

And it is this that the view which ascribes to products of cell action present within serum or plasma the property of being able successfully to wound, maim, and render harmless invading bacteria, and yet denies to the protoplasm of the cells which produce them the slightest power of controlling or in any way influencing the growth or virulence of bacteria. Such a view, I submit, is as utterly illogical as any could well be.

On grounds of logic I give decidedly the preference to the view which subordinates the product to the cell which produces it, and hence if I have to choose between the phagocyte theory which exalts the direct action of the cell without ignoring its more indirect action and the humoral theory which exalts to a pinnacle the action of bactericidal substances admittedly produced by cells, while denying to these same cells any special bactericidal action, I give my decision unhesitatingly for the doctrine of phagocytosis.

2. *Special Objections.*—The above does not constitute, however, the head and front of the offending of this doctrine in the eyes of its opponents. That which above all appears to them as gratuitous foolishness in it is, I take it, that it not only ascribes to cells, but to certain particular cells, this special phagocytic function. It is this pretentious claim on behalf of the leucocyte and lymphocyte that rouses the special scorn and indignation of the humoralists; and they express a contempt for these little structures that, could they but feel it, would cause them to shrivel up with shame at their own want of modesty and general aggressiveness of conduct towards bacteria.

Curiously enough, it is this special claim of the phagocyte theory, when considered in relation with the facts on which the humoral theory is based, that commends it specially to my acceptance, and strengthens its otherwise inherent probability—the inherent probability of the view, namely for which it contends, that the duty of defending the body against the attacks of infectious disease is not distributed amongst the cells of the body as a whole, but is relegated to certain special cells. These are arranged so as to constitute what has been termed two lines of defence. The first line of defence is formed by the leucocytes of the blood; the second by the lymphocytes and of follicular tissue generally, chiefly that of spleen. The former bear the first brunt of the attack at the seat of inoculation, the latter act as reserve forces. It is these cells on whose behalf I consider myself "retained."

According, then, to the doctrine of phagocytosis, acquired immunity is due to an altered habit of body on the part of these cells, transmissible from generation to generation. Its opponents, scorning such a view as visionary and unsatisfactory in the extreme, and based on insufficient data, prefer to refer it instead to a change in the character of the fluids of the body. This latter view implies an altered constitution on the part of the plasma and serum of the blood, without even suggesting in what way such an alteration has been brought about, except through the agency of cells.

Moreover, this alteration is one that in certain cases presumably lasts for years, and hence one of two conclusions follows: either that, once produced, it remains permanent—that blood plasma can transmit its characters without the intervention of cells; or that the change is one constantly being carried out by the cells of the body. The former, I need hardly say, is a wild hypothesis having no support either in facts or analogy; and therefore we must accept the alternative conclusion that, *even on the humoral theory of the nature of immunity, the essential change is an alteration in the cells, such as the doctrine of phagocytosis contemplates.*

This, however, it may be urged, does not carry us very far forward. We knew before, as Professor Sanderson has already pointed out, that the body possessed what he has termed a specific power of resistance to disease; and this *vis medicatrix naturæ* is naturally derived from its cells, if not resident in them.

The final teaching of the doctrine of phagocytosis would thus appear, as Mr. Kanthack

pointed out, to resolve itself into this: that what we formerly in our ignorance referred to the organism as a whole, we now refer to the individual cells of that organism. I have already replied to this objection so far as it applies to the cells of the body as a whole; I have now to submit that in going further, and referring to certain particular cells of leucocyte and lymphoid nature, a special *rôle* in combating infectious disease and in conferring immunity, the doctrine of phagocytosis is not only an advance, but constitutes, in my opinion, a great advance in our knowledge of the mechanism and nature of immunity.

I base this belief on grounds independent altogether of bacteriological studies—on data derived from my study of the physiology and pathology of the blood—and I will now endeavor to make these grounds clear.

The changes relied on by the opponents of the phagocytic doctrine of immunity affect in a special degree, if, indeed, not exclusively, the plasma and serum of the blood, and are to be referred to antecedent changes in the cells of the body as a whole, rather than in any particular group of cells.

In this view I recognize a great resemblance to one still widely held by both physiologists and pathologists regarding the constitution of the blood and blood plasma—namely, that the composition of the blood at any time is determined by the united action of all the tissues of the body.

As I had occasion to point out in the Arris and Gale Lectures of 1889, such a view is not supported by facts; the facts, on the contrary, point to a certain class of cells grouped for the most part in relation to a particular portion of the circulation as being primarily, if not exclusively, concerned with the constitution of the blood plasma—whether in building it up or in effecting alterations in it. These cells are the leucocytes of the blood and the mass of lymphocytes lying in relation to the portal tract, especially those of spleen and alimentary tract.

It is a coincidence to which I am inclined to attach not a little importance that the class of cells thus found to be mainly responsible for the constitution of the plasma is precisely the same group of cells found by the upholders of the doctrine of phagocytosis to possess phagocytic properties in a specially marked degree, and that the chief seats of phagocytic action outside the circulation itself are, as is agreed to by all, in the first place the spleen, and, as has been clearly shown by Ruffer, in the second place the alimentary tract.

Action of Spleen in Relation to the Blood and to Infectious Diseases:—It is not my intention to adduce here or now the evidence on which the above conclusion is based, so far, at least, as the leucocytes generally and the lymphoid cells of the gastro-intestinal tract are concerned. Suffice to say that I have always found changes in the constitution of the plasma associated with evidences of great activity on the part of these cells—nuclear division, increased fragmentation, and formation of granular *débris*. And as to the important part taken by these cells as phagocytes, I leave to others who afterwards speak in this discussion. In the hands of Dr. Ruffer and others, I have no doubt ample justice will be done to that *rôle*.

For myself I propose to confine my attention solely to the spleen, and endeavor to establish the justice of the claim made on its behalf by the doctrine of phagocytosis, that its cells have a special phagocytic function independent altogether of the scavenging function common to them and other active cells. In particular I hope to show that they have what is claimed for them and to a less degree for the leucocytes by the phagocyte doctrine a greater selective power than other cells of the body, and that this selective power is specially exercised by them in relation to causes tending to affect the constitution of the blood plasma.

The observations I am now about to refer to date from 1887. They were not made with any reference to their bearing on the doctrine of phagocytosis—a circumstance that should, in my opinion, enhance any value that may attach to them in this relation.

The evidence I would adduce from them relates to the action of poison on the blood (1) in the presence and (2) in the absence of the spleen. Briefly stated, it goes to show that the power possessed by certain drugs of altering the constitution of the blood depends primarily on the action of the splenic cells, and not on the cells of the body generally.

My experiments show for the poison toluylendiamin that its well-known destructive action on the blood is in an altogether special degree dependent upon the presence of the spleen; and that in the absence of that organ animals (rabbits) can tolerate about twice the dose without the slightest effect that would otherwise suffice to produce recognizable changes in the blood.

This difference in the action of poisons on the blood when the spleen is present and when it is absent does not apply to substances whose action on the blood is mainly phy-

sical, such as distilled water or glycerine, or, although to slightly less degree, pyrogallic acid. Nor is the power thus possessed by the spleen in the case of certain poisons due to any scavenging function on its part—the presence of a greater quantity proportionately of the substance in the spleen than elsewhere. On the contrary, by a method that enabled me to recognize the presence of even one-hundredth part of a milligramme of the above poison, and whose margin of fallacy I found to be by accurate control to be less than ½ per cent., I was able to show that the amount present in the spleen was less than in any organ of the body and less than that in the blood.

The result I interpret as showing what indeed other observations clearly satisfied me was the case that its injurious action on the blood was not a direct one, but an indirect one due to the intermediate action of certain cells, notably those of the spleen.

The bearing which these observations have on the subject now under discussion, the *rôle*, namely, of the cells of the spleen in modifying the constitution of the plasma of the the blood will, I trust, be obvious. For the subject of immunity and phagocytosis in relation to immunity involves a consideration not only of the action of cells on microbes, but also of the action of the cells on the products of these microbes. And these experiments appear to me clearly to establish that as regards these products, whose action can in no case be considered a purely physical one, their action on the plasma of the blood, for good or ill, must in a very special degree be dependent on the action of the splenic cells —of cells, which are claimed by the doctrine of phagocytosis to possess this phagocytic power in a special degree. *They clearly show at least for the products what the doctrine claims to be the case for the microbes themselves, that the splenic cells possess a special selective power that must render their action of the very greatest importance in the struggle against infectious disease.* And we are not without more direct evidence that such is the case. A series of experiments have recently (1889) been published, by Bardach, showing that the absence of the spleen affects materially the chances of recovery from infectious disease. His experiments were made on dogs, which are not naturally susceptible to anthrax even when inoculated subcutaneously; when it is injected into the blood they also resist, although a certain number succumb. After removal of the spleen they become much more susceptible. Thus out of twenty-five dogs whose spleens were removed, no fewer than nineteen died, while out of the same number of normal dogs similarly inoculated with anthrax only five died.

On the humoral theory of the nature of immunity it is difficult to understand how the absence of the small quantity of serum —to whose antitoxic and antibiotic properties recovery in such cases is ascribed—presumably present in the spleen could make a difference of such vital importance. Moreover, it is not easy to reconcile these facts with Dr. Klein's contention that even when phagocytes are present it is as probable that the microbes find themselves as comfortable within the cells as outside of them. If such were the case, the spleen ought really to be a favorable seat for the growth and multiplication of bacilli rather than the converse; and its removal should rather increase than diminish the animal's chance of recovery.

It must not be supposed for a moment as tending to weaken the force of this argument that the animal after the removal of its spleen is in an unhealthy state. On the contrary, except in respects of its power of resistance to the action of poisons and microbes, it appears to be in no way affected by the operation, as I have constantly had occasion in the course of my experiments to observe.

Time will not permit me to touch, as I should have liked, on the subject of chemiotaxis and its relation to phagocytosis and immunity. What I should have said I may compress into the few remarks that I may make in demonstration to you, as I now propose to do by means of lantern slides the chemiotactic properties of certain of the tox-albumoses I have isolated from tuberculin. The method employed to study the action of these substances was the one of which you will hear more from Dr. Ruffer, whose method it is. To Dr. Ruffer I am indebted for his great kindness in himself making the first obsevations with these substances for me; and I gladly take this opportunity of thanking him.

CONCLUSIONS.

In conclusion, I would formulate in a few words the chief grounds on which I think the doctrine of phagocytosis claims from us a greater credence than the one which has been opposed to it. I am disposed to claim for it that in revealing to us as it has done for the first time the potentiality possessed by cells of themselves combating the attacks of virulent microbes, it has added much that is not only new, but it is also valuable and important regarding the means by which infectious diseases are combated, and that over

knowledge of the bactericidal properties of blood serum, to which so much importance is attached by the opponents of the phagocyte doctrine, is itself the direct outcome of the attention drawn by Metschnikoff to the action of cells.

So far from being antagonistic, the two doctrines—the phagocytic and the humoral—mutually supplement and support each other; disproof of the phagocytic theory, so far from proving the correctness of the humoral, tends, in my opinion, to weaken it. Both theories recognize the existence of antitoxic and antibiotic substances of blood serum and blood plasma; but it is, in my opinion, the peculiar merit of the phagocytic as opposed to the humoral doctrine that it subordinates them in interest and importance to action of the cells from which they admittedly derive their origin. If a choice has to be made between the two views, the one we can least do without is that which attaches a preponderating importance to the cells of the body than to the fluids, to the action of the cells rather than to the products of that action, to the agents rather than to the instruments.

I am disposed to claim for the phagocytic theory that, while it does not profess, as its opponents allege, to be in any way a complete or satisfactory explanation of the phenomena of immunity, as a working hypothesis—and it is that we must first look for—it is in all respects more logical, more tenable, more in consonance with the teachings of cellular pathology, and I would almost say more biological than that which ascribes to the fluids of the body the first *rôle* in protecting it against infectious disease.

Considered apart from the teachings of phagocytosis—as the supporters of the humoral doctrine appear always desirous of doing—the humoral doctrine has as its chief characteristic, in my opinion, a self-satisfying sufficiency for which there is no real basis. It appears to explain, without explaining. It speaks much of antibiotic and antitoxic substances in body fluids, and is apparently content to rest there; it does not ignore their origin from cells, indeed cannot possibly do so; but, unlike the doctrine of phagocytosis, it does not encourage further inquiry into the nature of the changes of cells which lead to their production, into the conditions which determine the precise "reaction" in the cells necessary for their formation in the first instance.

On both views the final nature of the change, whether in cell or in product, is unknown; but as the change in the cell must necessarily precede that in the product, and the doctrine of phagocytosis directs most attention to the cells, that doctrine has in my opinion claims on our support out of proportion greater than any teaching that would even temporarily draw our attention away from the cell to the fluid in which it lies, that would exalt the product at the expense of the producer.

Lastly, I claim for the phagocyte doctrine that in attaching special importance to the action of certain cells—leucocytes of blood, cells of spleen, and lymphocytes generally—it draws attention to facts of wide physiological importance, namely, that it is precisely these cells that govern, in a special degree, the constitution of the plasma and blood serum. Whether acquired immunity be due to a direct phagocytic action of these cells, or to antibiotic or antitoxic properties of the plasma of the blood, the preceding changes must affect in a special degree these cells.

VII.—SIDNEY MARTIN, M. D.,
Assistant-Physician, University College Hospital.

Dr. Sidney Martin said that the question might be studied in two directions: (1) as to what rendered an animal naturally immune to a disease, and (2) as to the mode of production of experimental immunity. Immunity produced by experiment was a much better method of research than the investigation of natural immunity, inasmuch as the conditions of experiment were controllable.

Of the several examples of experimental immunity now known, that of anthrax was perhaps the best to be discussed, as the morphology of the bacillus anthracis was so well known.

In the classical experiments of Pasteur the attenuated bacillus anthracis when inoculated into a sheep was found to produce some digree of illness, which passed off. The animal was rendered immune, as the inoculation of a virulent bacillus one month after vaccination failed to produce death. What they wished to know was what happened to the virulent bacilli injected; why did they not kill the sheep as they did in an unprotected animal? Those who depended on phagocytosis for an explanation would say that the action depended on the activity of the leucocytes at the seat of the inoculation of the virulent bacilli, and that these leucocytes effected the destruction of the bacilli. This explanation was, to his mind, by no means sufficient. They had in the experiment just quoted first the injection of an attenuated living virus, and then

a subsequent inoculation with a virulent virus, also living. Now, as he had shown in his last paper in the Report of the Medical Officer of the Local Government Board (1890–1891), the attenuated anthrax bacilli formed the same chemical products as the virulent bacilli; but that whereas the attenuated bacilli would grow well for a time in a culture medium (such as alkali-albumen broth) in which they could form the products, after a certain period they stopped growing; the virulent bacilli would, in a similar solution, go on growing for a much longer time, and would produce a much larger quantity of chemical products than the attenuated. A bacillus, whether attenuated or virulent, acted by means of its chemical products. In the case, therefore, of Pasteur's sheep experiment, it must be said that immunity was produced by means of the chemical products of the attenuated bacilli, and yet the virulent bacillus which produce the same products did not kill the animal. Its natural growth and activity had been inhibited. How could this be explained? Part of the explanation appeared to be that the inoculation of the attenuated virus accustomed the animal to the specific chemical poison of the bacillus. Several instances of tolerance of drugs, such as morphine and arsenic, were well known in pharmacology—a tolerance which, when established, would enable the person to take with impunity a dose of the drug which would be fatal to an unprotected person in a few hours.

A certain degree of tolerance to virulent anthrax bacilli might be established in animals by means of the chemical products of the micro-organisms, as in the following experiments quoted:

A guinea-pig was inoculated under the skin of the right thigh with 10 milligrammes of the anthrax albumose and 9 milligrammes of the alkaloid; under the skin of the left thigh, with a "mash" of the spleen of an animal which died of virulent anthrax. This dose of the chemical products produces only a slight physiological effect in guinea-pigs. The animal lived four days and a half, whereas the control animal died within twenty-four hours. The lesions found were those of anthrax. In a second guinea-pig, done at the same time, the same dose of chemical products and of "splenic mash" were mixed together and injected under the skin of one thigh. This animal showed no change for twenty days, when it died somewhat suddenly. A *post-mortem* examination showed no œdema at the site of inoculation, and only a slight enlargement of the spleen, which was very pale.

In the first experiment, he should say that part of the explanation of the delay of death was that the chemical poisons influenced the organs (of the central nervous system) which they selected for their action, and that the effect of this action was to prepare them for the onset of the larger amount of products produced by the virulent bacillus, but that the alteration in the tissues was too slight to prevent death. Such an experiment was a very imperfect imitation of what occurred after the inoculation of an attenuated living virus, for he thought that the products of the attenuated bacillus, being gradually formed, were gradually absorbed, so that the physiological effect was one of repeated small doses of the specific poison, the action lasting for a long period of time. That the chemical products of pathogenic bacteria were a long time in the body he knew from his own experiments. A single dose of the anthrax albumose would produce fever lasting five days, and with the products from other diseases he found evidence of their presence in the blood ten, or even twenty-four days after inoculation. It was probable, therefore, that the chemical products of attenuated bacilli would remain longer in the body than when the chemical products were simply injected, and he had but little doubt that this retention of specific chemical substances in the blood (which they profoundly affected) had an important bearing on immunity.

Why, it might, however, be asked, did not the virulent bacillus grow in the artificially immune animal? Even supposing the chemical products of the attenuated virus were present in the body at the time of inoculation with the virulent bacilli, could they be supposed to stop the growth of the bacilli, or weaken them so that they behaved like the attenuated virus in having a short existence? The second experiment appeared to bear on this point, for mixing the chemical products with the bacilli delayed death for twenty days, while in the first experiment, where the injection occurred once on each side of the body, death was delayed for only fourteen days and a half. Now, if virulent bacilli were grown in a flask containing a nutrient medium from which they could form their products, after a time the growth stopped, just like peptic digestion, where the products choke the action of the ferment. It was hardly possible, he thought, that the small amount of products which might be present in the body after the injection of the attenuated virus could choke the growth of the virulent bacillus by their own action, but it is not improbable that the profound effect which they

had on the blood might diminish the advantages which that liquid possesses of serving as a nutrient medium for the bacillus. He did not pretend that what he had stated explained immunity, but they were matters for thought and for future experimentation, as showing that there was something beyond phagocytosis in the question of immunity.

VIII.—ALMROTH E. WRIGHT, M. D. DUB.,
Late Grocers' Research Scholar

Dr. Wright said he did not intend to enter upon the main issues of the debate, as he had no original observations to communicate which had a direct bearing upon phagocytosis as a method of resisting disease. Without, however, taking a side as a "phagocytist" or a "humoralist," he might perhaps be permitted to bring into hotchpot a few observations upon the subject of chemical immunity which might turn out to be pertinent to the matter in discussion.

The history of the subject he had been engaged upon was shortly as follows: The late Dr. Wooldridge, in 1888, described a method of what he termed "chemical protection" against anthrax. He found that when he administered tissue fibrinogen to rabbits they remained immune against a subsequent injection of anthrax. He employed tissue-fibrinogen solutions, which were prepared either from the thymus or from testicles, and which he subsequently boiled in order to render them sterile. His results did not appear to have been uniformly successful, and he seemed to have attributed the failures in great part to the fact of his protective fluid becoming inert during the process of boiling. Dr. Wooldridge died, and his work was left in this incomplete condition.

Mr. Hankin, of Cambridge, in whose mind a somewhat similar idea with regard to to chemical protection had been simmering before the date of Wooldridge's publication, was the next to take up the matter. He adopted the term "defensive proteids" for the albuminous substances in which protective chemical properties were supposed to be resident, and he proceeded to graft the idea of a bactericidal power on to the more generic conception of a defensive proteid. He suggested, for instance, that if Wooldridge's tissue fibrinogen really conferred an immunity against anthrax, it probably conferred that immunity by virtue of a bactericidal property with which it was endowed. Dr. Wright gathered that Mr. Hankin had since abandoned this suggestion.

The next observation of Mr. Hankin which he wished to bring into relation with Wooldridge's work was his observation that a particular substance—or more probably mixture of substances—of protective virtue could be extracted from the spleen of the white rat—that is, from the spleen of an animal which was known to be relatively immune against anthrax. This substance or mixture of substances, under the name of an alexin, was, if he understood Mr. Hankin aright, to be considered responsible for the measure of immunity which the white rat possessed over the more susceptible brown rat. He would endeavor to deal very briefly with the work of those two observers.

With regard to Wooldridge's work, it was obviously necessary to reopen the investigation of the subject by repeating his experiments, and then proceeding to isolate the constituent to which the immunity was due. It also seemed desirable to adopt some more appropriate method of sterilization than that of boiling, for Wooldridge himself had recognized the disadvantages which were associated with this method.

Dr. Wright attempted to do these three things in a paper which was read at the International Congress for Hygiene last year.* He there showed that sterilization by means of a Chamberland's filter might with advantage be substituted for the method of heat sterilization. He further showed that tissue fibrinogen was a proteid substance which belonged to the class of the nucleo-albumens or nucleo-proteins. Lastly, he reported several series of experiments in which he succeeded, by means of tissue-fibrinogen injections, in prolonging or preserving life in rabbits which had been inoculated with virulent anthrax.

Certain defects in his experiments in the matter of preserving his tissue-fibrinogen solutions in an absolutely aseptic condition throughout the experiments he had since remedied, and had obtained exactly the same results with absolutely sterile solutions. The results of his experiments on anthrax were that he had never made a series of experiments without obtaining either a prolongation of life or a complete cure by means of the tissue-fibrinogen injections. The results were, however, as yet by no means satisfactory, and there were difficulties to be contended with in the matter of the attenuation of the tissue-fibrinogen solutions during sterilization by the retention of the tissue-fibrinogen on the pores of the filter.

Having got thus far, he was naturally anxious to obtain some information as to the

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manner in which this curative property of tissue fibrinogen became effective. He found, in conformity with Wooldridge's previous observations, that anthrax could be got to grow in tissue-fibrinogen solutions. He therefore concluded that whatever the effect of tissue fibrinogen might be due to, it was clearly not due to a bactericidal power. Mr. Hankin, he believed, agreed with him in this.

Dr. Ruffer had suggested to him at the outset of his work that tissue fibrinogen might possibly act as a cell stimulant. From this, and from a paper of Groth* upon the effects of an intravascular injection of leucocytes he had turned his attention to the examination of the blood and to the enumeration of the leucocytes after tissue-fibrinogen injections. These enumerations gave at once some very tangible results. In every case the injection of tissue fibrinogen was observed to be followed by a remarkable increase in the number of leucocytes. He had repeatedly seen them increased to as much as six times their normal number, and at the same time a remarkable poikilo-leucoytosis made itself felt, some of the corpuscles being of enormous size, and others, again, of very diminutive size. At this stage of the paper he came across the very remarkable paper of Horbaczewski's† upon the physiological effects of nuclein.

Now, since he had shown in his previous paper that nuclein was a constituent of tissue fibrinogen, and since Horbaczewski proved that the admistration of this nuclein resulted in the production of a leucocytosis, he naturally turned to investigate whether the protective effects of a tissue-fibrinogen solution could be obtained by an adminstration of its nuclein moiety alone. He anticipated that he should incidentally reap the advantage of an exact dosage, and also an advantage in the matter of obtaining aseptic solutions with greater facility. He had only just begun the investigation of the matter, but had already been able to satisfy himself that a very remarkable prolongation of life resulted from he administration of the nuclein before an anthrax inoculation, and that this prolongation of life was associated with an increased leucocytosis.

In what relation did this chemical protection stand to Mr. Hankin's researches on immunity? Mr. Hankin obtained his alexin from the spleen of the relatively immune white rat. He obtained it by extracting it with what was practically a normal saline solution. If there were any tissue fibrinogen present in an organ a watery solution would extract it, and in the spleen, where there were leucocytes in relatively large quantities, the main constituent of leucocytes, that was, tissue or cell fibrinogen, must of necessity be present. Therefore Mr. Hankin's extracts of white rat spleens would contain tissue fibrinogen. Mr. Hankin's next proceeding was to precipitate his watery splenic extract with alcohol, redissolve the resulting precipitate in water, and to employ this last extract, after dialysing away the salts, as a protective fluid. The method of preparation was an almost ideal method for ensuring the presence of tissue fibrinogen in Mr. Hankin's final solution, for while the alcohol precipitation might exclude other elements which might be present in the splenic extract, it certainly could not exclude the tissue fibrinogen, for this substance was not rendered insoluble by alcohol.

He thought, therefore, that Mr. Hankin's alexin was the same thing as Wooldridge's tissue fibrinogen, or at any rate that it could not fail to be contaminated with it. In this connection he would point out that the nuclein with which Horbaczewski's experiments were conducted was prepared from the leucocytes of the spleen. With regard lastly to Mr. Hankin's observation that his alexin was found only in the spleen of the relatively immune white rat, he suggested that this might possibly depend upon a greater development of the adenoid tissue of the spleen in the white rat, for it was well known that the spleen played an important part in the conflict of the organism with the micro-organism of disease. A greater development of adenoid tissue would go hand in hand with a larger percentage of tissue fibrinogen in the extracts.

In conclusion he adverted for a moment to the newer development of the Metschnikovian doctrine which aimed at the reconcilement of the opposing schools of phagocytists and humoralists by assuming that the disintegration products of the white corpuscles played a part in the conflict of the organism with infective disease. Each of these chemical disintegration products might possibly act in a protective manner by virtue of either a chemiotactic power, a bactericidal power, or a cell-stimulating power.

Dr. Ruffer had determined for him that tissue-fibrinogen solutions had a powerful positive chemiotactic power. The observation seemed to him of interest as it might help to explain how fresh leucocytes might be attracted to the seat of infection the bacteria

Dessert, Dorpat, 1884.

†*Sitzber. d. Wiener Acad.*, C. Abth., April, 1891.

having gotten the better of the first leucocytes which had arrived on the scene.

With regard to the bactericidal power of tissue fibrinogen, it seemed to him that the question had not finally been decided in the negative as his observations on growing anthrax in tissue-fibrinogen solutions were made upon tissue fibrinogens derived from the testicle, and there might conceivably be points of difference between the two varieties of tissue fibrinogen. On the other hand, he thought that great caution was required in inferring from a bactericidal property of a solution in a test tube to the bactericidal power of the same substance in the organism, because such a substance, for instance, as tissue fibrinogen was rapidly broken down inside the organism.

With regard to the cell stimulating powers of the products of the decomposition of leucocytes, it has already been seen that tissue fibrinogen possessed such powerful stimulating properties. He thought that tissue fibrinogen and nuclein might be regarded as cell manures; for by whatever method these substances were introduced into the organism, they were invariably found to produce a leucocytosis.

IX.—J. G. ADAMI, M.A., M.B.,
Fellow of Jesus College, Cambridge.

When the time allowed to each participant in this evening's discussion is necessarily so short, perhaps the best method of attacking this subject of immunity is to start with the best and most complete series of observations in the case of one disease, observations that are both recent and reliable, and then, accepting the facts so gained, to observe how they oppose or confirm the various theories which have been brought forward. For this purpose I would select the very full and pregnant research of Klemperer upon pneumonia. The result of these observations may be briefly stated as follows:

1. Immunity against pneumonia can be bestowed upon susceptible animals by introducing in the tissues the sterilized products of growth of the pneumococcus. This immunity is, in general, but of a temporary nature.

2. Such immunity induced by injection of bacterial products does not immediately manifest itself; indeed, fourteen days must elapse before the simple products bring about their effects. But if the sterilized products be heated either to 106° or 107.5° F. for three or four days, or to 140° F. for two hours, then injections induce immunity within four days.

3. The warmed "vaccine" leads to a reaction of but short duration; the unwarmed brings about a long-continued febrile state at the end of which the animal becomes immune.

4. Thus the fever, with its elevation of temperature, and the antecedent heating of the vaccinal material induce the same result. One or other process would seem to be necessary in order that sterilized bacterial products develop immunity.

5. But, further, the blood serum of a "protected" animal injected into the veins of a susceptible animal confers immediate immunity. There is no delay. Evidently, therefore, some substance is present in the blood serum of an animal made immune—a substance not present in the sterilized culture fluids of the pneumococcus. The bacterial products, therefore, do not *per se* bring about immunity, but, through the febrile reaction, some other body, having immunity-conferring properties, is gradually developed.

6. What is more, this same substance has curative properties, acting, not so much on the pneumococci themselves (for in its presence these continue to proliferate), but upon the poisons or toxines manufactured by them.

7. Hence Klemperer distinguishes two proteids, the pneumotoxine, which can be isolated from cultures of the pneumococcus, and the anti-pneumotoxine, to be obtained from the serum of immunized animals.

It is unnecessary for me to dilate upon the way in which this work of the two Klemperers fulfils and elucidates the previous observations of Salmon and Smith, Christmas, Roux and Chamberland, Hankin, Bouchard, and his school, Brieger and Fraenkel, etc., upon bacterial products, of Wooldridge, Christmas, Hankin, Buchner, Hankin and Martin, Tizzoni and Chattani, and Fraenkel upon proteid bodies capable of inducing immunity, and, in the case of some of these observers upon the antitoxic properties, as opposed to the bactericidal properties of such proteids. Nor need I do more than point out the light that these observations throw upon the meaning of the febrile state. Evidently, as shown by a large number of recent studies, the phenomena of infection and immunity are at base chemical; certain substances are produced by the bacteria on the one hand, by the organism on the other, whose action is of a chemical or chemico-physical nature.

But now comes the question, how is this action exerted within the body? Are bactericidal and antitoxic substances normally free in the blood, are they developed within

special cells, or are they generally distributed throughout the tissues of the body?

In the first place it cannot be too strongly insisted that their presence in blood serum is no certain indication of their presence in blood plasma. Blood serum and blood plasma exhibit marked differences in their characters, and nothing is more marked than this difference in the bactericidal powers of the two fluids. An animal which is most susceptible to a given disease has time after time been shown to yield a serum capable of destroying large numbers of the micro-organisms causing that disease; and, at the risk of repeating what is well known to many, I would again call attention to Lubarsch's classical observation, that whereas 10,000 to 12,000 anthrax bacilli are all that can be injected into the circulation of a rabbit without causing a fatal disease, one cubic centimetre (15 minims) of the blood serum of such a rabbit will in a few minutes destroy the same or a much greater number of the bacilli. While I am far from convinced that under no conditions the plasma can become bactericidal or antitoxic, I still think that sufficient satisfactory evidence has not yet been brought forward in support of the humoral theory. The cases mentioned by Professor Klein are not without a flaw; the rapid diminution of the number of bacteria present in arterial or venous blood is not conclusive evidence that these bacteria have undergone destruction in the blood plasma (phagocytosis being as yet undeveloped). Professor Klein and the humoralists have to bring forward surer proof than has yet been adduced that the bacteria, as foreign bodies, have not been arrested in the capillaries of various organs, and that the diminution of their number in the circulating blood is not caused by such a filtration process.

Again, while I am prepared to admit freely that bactericidal and antitoxic substances may appear or be developed in the cells forming many of the tissues of the body, I hold that it has yet to be proved that this can obtain to any great extent. We are still without proof that, save in the case of organs formed largely of leucocytes and potential phagocytes, the tissue can have powers at all comparable to those possessed by the serum and body fluids. We therefore are driven to a third possibility, namely, that the difference between serum and plasma depends upon an alteration in the condition of the white corpuscles, and that the leucocytes are a most essential factor in the production of immunity. There is not time here to bring forward all the arguments in favor of this view, but this I may say: what other interpretation is to be placed upon (not simply the engorgement, but) the enlargement of lymphatic glands, the enormous increase in the number of white corpuscles in the blood in zymotic disease (under conditions to be referred to later), the determination of leucocytes towards legions of bacterial invasion, unless it be that the leucocyte plays a most important *rôle* in protecting the organism? It is true that there are conditions in which it is wanting. In pneumonia, for example, as Tchistovitch has shown, where the virus is not very powerful there an increase occurs, where the virus is very strong there a diminution is to be made out in the leucocytes of the blood. The same occurs in regard to the amount of diapedesis resulting after local invasion, this depending upon the virulence of the invading organism. Taking into consideration general rather than local infections, we learn that the leucocytosis is dependent upon the reaction of the organism to the soluble products of the virus.

That the leucocytes can possess bacteria-killing powers is supported by Hankin's observations upon the lymphatic glands and spleens of refractory animals, in which he showed that from these organs, composed as they are largely of mature and immature leucocytes and the parent cells of the same, can be obtained a bactericidal substance acting specifically upon the microbes to which the animal was refractory. It is impossible for me to criticise Dr. Woodhead's objection to this—namely, that Dr. Wright has grown micro-organisms freely upon leucocytes—until the conditions of these experiments have been fully described.

To come now to the assumptions of bactericidal powers by blood serum, I would remark that, as Reuschenbach has shown, and as a matter of common experience, coagulation of the blood and production of serum is accompanied by a great disintegration of certain of the white corpuscles,* and I would suggest that to this disintegration is due the presence of bactericidal substances in the serum. Just as from fresh blood, and intact white corpuscles no fibrin ferment is obtainable, so it may be that the intact leucocytes do not *normally* contain bactericidal substances but precursors thereof. Possibly this is fitted to explain Dr. Woodhead's contention with reference to quinine.

I am unwilling that this working hypothesis whereby I have attempted to reconcile for the time being the divergent results of a host of capable workers should be dubbed a theory,

*Upon these points see fuller details given by Dr. Wright in the *Lancet* of February 27th.

still more do I regret that it should have been accredited to me; for while I have worked it out along my own lines, its origin is to be found in Mr. Hankin's work, and to all intents and purposes this is the hypothesis brought forward by Hankin at the International Congress of last year. If for no other reason, I am glad to join in this evening's discussion in order that honor may be given where honor is due.

Lastly, as to the way in which the leucocytes act upon the bacteria, I cannot help being impressed by the fact that the more perfect the methods employed the greater is the phagocytosis observable in the course of resolution of disease; and I must conclude that even though leucocytes have other methods of acting upon bacteria and their products, phagocytosis is a most important, probably the most important, factor in the production of immunity. Professor Burdon Sanderson's main objection—that leucocytes are to all intents and purposes independent organisms, and that, therefore, they cannot reasonably be expected to act in accordance with the needs of the organism—is, it seems to me, based upon the idea that the leucocytes are generation after generation propagated in the blood, free from the fixed cells of the body, But this is scarcely the accepted view. Very little mistosis of the leucocytes occurs in the blood, whereas the characteristic mistotic areas in the lymph glands offer an almost certain indication that the proliferation of leucocytes occurs in regions where the parent cells may be regarded as fixed, and capable of being influenced by nervous and other stimuli. It is to these parent cells, rather than to the free, motile leucocytes, with their short span of life, that we must look to explain continued powers of resistance to bacterial products on the part of leucocytes, the development of increased resistance, the development of immunity.

X.—T. J. BOKENHAM, L.R.C.P., M.R.C.S.,
Research Scholar British Medical Association.

Mr. Bokenham gave the results of some investigations on erysipelas which would have to be explained before the question of the causation of immunity could be settled: (1) Different effects of injecting cultures of true steptococcus erysipelatis into rabbits and guinea-pigs. Inoculation of a virulent culture into a rabbit's ear produced a typical spreading erysipelas without pus formation, though the lymph, whilst the process was advancing, was very rich in leucocytes. If some of this lymph were injected into a guinea-pig and another rabbit, the latter would contract a disease in every respect similar to that in the first rabbit, but nothing would happen to the guinea-pig. (2) Alteration of virulence of streptococcus erysipelatis under varied conditions: If a culture of streptococcus erysipelatis (too attenuated to produce the disease in a normal rabbit) were injected into one into which a simultaneous intraperitoneal injection of a sterilized culture of bacillus prodigiosus had been made, typical erysipelas would appear at the point of inoculation.

In this case a rabbit which was immune to the erysipelas of a low virulence was made susceptible, and inoculation from it to a normal one would produce the typical disease in the second; and, by continuing the process through successive rabbits, a very virulent form of erysipelas could be produced in which a highly infective disease occurred with generalized symptoms, though no reaction occurred at the point of inoculation. Here undoubtedly the local cellular infiltration seemed to have played some part in the localization of the disease. If a rabbit vaccinated against the bacillus pyocyaneus were inoculated with streptococcus erysipelatis, a general fatal infection occurred without local reaction at the seat of inoculation, but blood from this rabbit could only produce the local disease in a normal rabbit. Association of attenuated erysipelas with a common mould increased the virulence of the streptococcus both generally and locally.

HARD CHANCRE OF THE LIP AND SOFT CHANCRE OF THE GENITALS.

Dr. P. I. Messarosh relates, in the *Meditsinskoye Obozrenie*, No. 17, 1891, the case of a young man, 20 years of age, who presented himself at the Astrakhan General Hospital for treatment of a sore lip. Examination revealed the presence of a true Hunterian chancre on the upper lip, and also a soft chancre of the penis. The patient stated that he had accidentally bitten his lip some time before, and that soon thereafter a sore appeared which refused to heal in spite of numerous domestic remedies which he had applied. While the man was still under treatment a woman presented herself for treatment for syphilis. She was found to be suffering from general syphilis, having mucous patches in the mouth, and also from soft chancres in the vagina. This woman, it was subsequently ascertained, was the mistress of the other patient, with whom he had lived for several months, and from whom undoubtedly he had acquired his lesions.

PUNCTURED FRACTURE OF THE SKULL TREPHINE AND RECOVERY.

By A. D. BINKERD, M. D.,
WEST MONTEREY, PA.

On Sunday about 10 o'clock A. M., Sept. 13th, I was called to see H. L——, who had jumped from a rapidly moving train and sustained serious injury. He was picked up within fifteen minutes, unconscious and bleeding from a small scalp wound over the occipito-parietal suture, a little back and probably two inches above the tip of the ear on the left side of the head. It seems highly probably that the car struck the patient after he jumped, and that some sharp point produced the punctured fracture. There was a severe contusion of the deltoid muscle with abrasion of the skin on the upper third of the arm on the same side. Thorough examination discovered no fracture other than a small puncture and angular depression of both plates of the skull beneath and a little forward of the clean laceration of the scalp.

There were evident symptoms of both concussion and compression of the brain. The pulse was slow and feeble. Respiration twenty-four. The patient vomited and remained in a semi-comatose condition all night.

In the morning I telegraphed for Dr. R. S. Wallace, of East Brady. On his arrival he confirmed my diagnosis.

After a liberal use of Squibb's ether, Dr. W. proceeded to remove a button with the trephine, just beneath the puncture, but so sharp and angular was the depression that we found it impossible to raise the plates without removing them bodily, which we did.

Little spiculæ of the inner plate had been thrust into the dura mater. These were carecarefully removed. The hæmorrhage was profuse and the operation necessarily consumed considerable time owing to a scarcity of trained assistance.

When the work was done the convolutions of the brain were exposed, fully three cm. by two cm. in extent.

The hæmorrhage was kept under control and the integument brought into apposition by sutures, the whole being done antiseptically, as nearly as possible.

In due time the patient rallied from the anæsthetic, but symptoms of compression continued till the next day, when they gradually disappeared. There was perceptible dilatation of the pupil on the side of lesion.

Fifteen grains of the bromide of potassium with three drops of tincture of aconite root every three hours with cold compress were agreed upon as the treatment for the first few days. This was strictly carried out. Second day we began calomel, four grains every four hours; this was continued for several days, keeping a sharp lookout for the blue line. A low diet and absolute quiet were required. Kidneys acted well, but there was torpidity of the bowels.

About the fourth day fever began to rise. Temperature 104.° Pulse 120. Respiration 26. Symptoms of herniæ cerebri, so much dreaded by Dr. D. Hayes Agnew, began to threaten as a new complication. It now happened that we had a very hot day, being nearly the hottest in September. The effect upon the patient was severe and to some extent alarming. Arterial hæmorrhage was controlled with difficulty at one point only to burst forth anew at another with increased vigor. A tumor, the size of a hen's egg, had forced itself through the wound. This had little tendency to calm the fears of the friends and parents. Calling to my aid the "lost art" of phlebotomy, I took blood from the arm in a full stream, till the patient expressed relief from pain.

The battle for ascendency continued for four days and nights, when improvement began.

The bromide and aconite were continued. In due time the bowels were evacuated, and the secretions established. Milk and bread constituted the chief diet. A ripe peach was allowed daily, then ripe cantaloupe or musk melon. During the second week thoroughly boiled wheat served with cream and sugar aided in keeping up gentle action of the bowels.

Delirium and incoherence were controlled by fifteen grain doses of chloral hydrate with bromide. A three to five grain capsule of quin. sulph. daily constituted the tonic from the second week.

The wound was thoroughly cleansed twice daily with standard antiseptics. Very little suppuration occurred.

The dropping of the scalp over the abrupt edge of the skull now gave us some concern, and the effort to prevent this natural phenomenon by inserting a pin, and thus bridging the scalp, did not result in any apparent benefit to the cicatrice.

The wound had now diminished to the size of a large corn grain, but refused to recede farther. One day while cleansing the wound I discovered a hard white substance which I could not readily remove. The next day I took it away. It was a fragment of the inner plate 1½ in. long and ¼ of an inch wide. Some ten days later I removed another small spicula.

The young man had begun to gain in weight, slept well, with increasing appetite, and had general good health.

Dizziness in walking was not a prominent feature. But he has as yet no recollection whatever of the occurrence of the accident.

The contusion of the shoulder resulted in resolution by suppuration, and showed a marked sluggishness in healing, yielding, finally to iodine injections supplemented by the seton.

This young man is 20 years of age, and was in robust health at the time of the accident. He has been at work in a rolling-mill in Pittsburgh since the first day of January, 1892, being less than four months after the occurrence of the accident.

Though we advised him strenuously against the resumption of physical labor till a considerable time had elapsed, he says he feels about as well as he ever did.

At this writing, careful inquiry elicits the fact that there exists a perceptible transudation from a small area beneath which it is highly probable a small fragment of dead bone still remains.

That the natural covering of the convolutions of the brain which we removed by the operation has been in part restored by analogous tissue is highly probable. Whether the process of restoration will continue to completion and possible compression of the subjacent convolutions of the brain, or whether the arch will be sufficiently concave to prevent such compression and its consequences, is the important point yet to be determined in this case— a point in which all brain surgeons must be vitally interested.

REPORT OF A CASE OF OVARIOTOMY.*

By DRS MARTIN AND CRAWFORD,
KAHOKA, MO.

Jan. 3d, Mrs. B——, aged 61, consulted Dr. Martin concerning a growth or tumor in the abdomen, which was of rapid growth and producing a variety of distressing symptoms. The abdomen was enlarged to the size of full term of gestation, the uterus was crowded down into the vagina; the breathing was interfered with from pressure against the diaphragm; the bladder could not be emptied except while in recumbent position, and the patient would then lift the tumor with her hands so as to remove the pressure from the neck of the bladder, and allow the urine to dribble away while in this position. These, with other distressing symptoms, were sufficient to cause the patient to willingly consent to the performance of an operation for the removal of the tumor, which we diagnosed an ovarian fibroid cyst.

We began to prepare her for the operation by giving her supporting and tonic treatment, with saline cathartics to free the bowels the day previous to the operation, enjoining daily baths and strict cleanliness of clothing and bed. On Jan. 14th, with the assistance of Dr. H. W. Stover, of Kansas, and other necessary help, we removed the tumor by first using all the care possible with regard to complete antisepsis, believing that success in all cases of laparotomy depends more upon thorough and complete antiseptic measures than any other one thing.

Our instruments and everything to be used were first placed in boiling water and afterward submerged in a solution of bichloride of mercury; the hands of operator and assistants were bathed in the same. The anæsthetic used was a combination of chloroform, ether and alcohol, one and a-half ounces of whisky being given a few minutes prior to administering the anæsthetic, which was given her while in bed. She was then placed on the operating table, and the operation commenced by making an incision from immediately below the umbilicus down to the linea alba to pubic arch, through integument, fascia and muscles to the peritoneum, waiting a few minutes to use sponge and ascertain if any vessels claimed attention. Just here we will say, that less than one ounce of blood was lost in the entire operation. The peritoneum was cut through and the tumor brought to view; it was found to be adherent to peritoneum and slightly so to intestines.

Notwithstanding the incision was six inches long, we decided to puncture the sack before removal, which we did and drew off about four quarts of gelatinous fluid, greyish in color and of the consistency of strained honey.

The adhesions were then broken up and the tumor lifted out of the abdomen by the hands of the operator; its attachment was then found to be the broad ligament on the right side of the uterus. The pedicle was two inches long and about two and one-half inches wide; we now passed a large needle, armed with double silk ligature, through centre of pedicle and tied from both sides. After which, we used a heavy ligature encircling the entire pedicle; we then excised and laid the tumor aside, sponged out the abdominal cavity, examined the stock to see if any bleeding ensued and immediately began to

*Read before the Tri-State Medical Society at Keokuk, March 1, 1892.

close the wound by fixing the pedicle at lower angle of incision and fixing it firmly there by pins and sutures; continuing sutures every inch or less until the incision was entirely closed.

After drying the body, we placed a layer of absorbent cotton, previously baked in the oven, over the incision and placed a bandage of new white flannel neatly about her and laid her back in bed where just thirty minutes before we had taken her out for the operation. She endured the anæsthetic well and the shock soon passed off, so that only a few minutes after she reached her bed she regained consciousness, and one hour later felt comfortable and took nourishment. We allowed no solid food for fifteen days, held the bowels in check for eight days, at which time we used injection and bowels responded nicely. The only medicine given was an anodyne of Tully's powder every ten or twelve hours; we saw her daily for seven or eight days and occasionally thereafter for the next three weeks. At no time did the temperature rise above 100°, and the pulse was at no time above 90. Soreness was slight from the beginning and no actual pain was experienced, the incision healed by first intention. Patient slept well and expressed herself as having felt more comfortable at the end of the first week than she had for seven months prior to the removal of the tumor. After ten days the patient began to sit up in bed, could turn herself without assistance, and now, at the end of thirty days, can walk about the house, and is entirely well.

We did not weigh the tumor, but believe it would, together with contents, have weighed 13 or 14 pounds. The usual weight of the patient is about 90 pounds.

APPOINTMENT OF RESIDENT PHYSICIANS AT EPISCOPAL HOSPITAL.

An examination for the position of resident physician in the Methodist Episcopal Hospital, of Philadelphia will be held on Tuesday, March 29th, 1892, at the hospital, Broad and Wolfe Sts., Philadelphia, at 8 p. m.

The examinations is open to both men and women; but applicants must, in accordance with the charter of the hospital have the degree of Bachelor of Arts. Two residents will be selected by the Trustees from the candidates who obtain the first four places in the examinations by the Medical Board.

Applications to be made to

Dr. John B. Roberts, 1627 Walnut St.
Dr. Richard C. Norris, 1028 Spruce St.
Dr. Wm. C. Hollopeter, 1408 N. 13th St.
Dr. H. H. Kynett, 1728 Spring Garden St.

SOME REMARKS ON SCARLET FEVER.*

By A. F. HEISING, M. D.,
MENOMINIE, WIS.

The etiology of this dreaded disease is no doubt due to a specific germ not yet perfectly isolated. The poison having entered the system either by ingestion or inhalation permeates the blood and the contents of the miliary vesicles, as experiments on healthy individuals prove. It has been observed that the contagion is manifested throughout the course of the disease, especially when desquamation takes place, the viability of the germ extending even from 5 to 8 weeks after the cessation of the disease.

When we speak of scarlet fever, we invariably consider it a disease of childhood, yet even adults, as your own experience proves, are not entirely exempt. Women in the puerperal state seem very susceptible to it, and I have no doubt that many a time a physician has mistaken this disease for some septic condition. Children having open wounds, as observation teaches, are more liable to take it.

It is a common opinion among the laity, that scarlet rash and scarlet fever are quite different diseases; yet in what really does the difference consist but in the intensity of the poison?

The prodroma are uncertain, usually a sudden chilliness with angina seems to initiate the disease, followed by vomiting and occasionally convulsions. On the second or even at the close of the first day a characteristic eruption of small red dots closely crowded together, makes its appearance, first on the chest and face, becoming general as the disease progresses, reaching its limit on the fourth and fifth day.

The temperature on the third day is usually the highest, subsiding from the fifth to the ninth day by lysis, unless other complications arise. Comparing pulse and temperature, the former is rather rapid. Next to the temperature, the throat symptoms are almost invariably present, although I have heard of cases in which they were absent. The severity of the throat symptoms may differ from an ordinary angina, with enlarged mucous follicles and suppurating portions of tonsil to the diphtheritic form.

Last spring diphtheria almost decimated its victims in our city, at the same time scarlet fever also prevailed, and I noticed that the throat symptoms of scarlet fever

*Read before the Inter-County Medical Society, March, 1892.

patients were by far more severe. I cannot help but think that many a time the two are closely allied, but in those cases assuming the diphtheric form, I never noticed the occasional paralysis which follows diphtheria. If a person can have diphtheria and scarlet fever simultaneously, one thing is certain, that while the scarlet fever is aggravated the characteristic diphtheria is mitigated.

Whether the Klebs'-Loeffler bacilli and those of scarlet fever antagonize each other, future experience and study will reveal. Bacteriologists teach us that we are surrounded by countless microbes, that almost threaten to devour us while our system is unaware. That they may be parasitic and thus antagonize one another, I would not be willing to say; but if this be true, that a person may have both diseases simultaneously and that one mitigates the other, who knows but many others may be found that will act in the same way and open up a new avenue of treatment?

The swelling of the glands in the neck, followed occasionally by an abscess, is of no rare occurrence, especially in the diphtheritic form. It seems strange that children having purulent otorrhœa after scarlet fever very seldom have meningitis.

Considering the elimination of the disease partly through the urinary tract, we ought to be prepared for the kidney complication, thus a timely examination of the urine in the third week may warn us of the approaching complication.

The fact that patients may have had the disease lightly does not vouch for the escape of nephritis. I remember a family in which eight children were taken down with the disease, one of the number had it very mildly, but he suffered with the worst uræmic convulsions and vomiting I ever saw, necessitating almost all medicines to be administered hypodermically.

I would call your attention to the fact that in the early stage of the nephritic complication, I have noticed pains in the abdomen opposite the kidneys. This may be misleading at times, yet it is a fact that pains are not always felt in the organs affected.

The pneumonia noticed in scarlet fever seemed to be more the result of kidney complication. Friëdlander has found that an acute nephritis of only a few weeks' duration resulted in hypertrophy of the left ventricle.

It is a peculiar fact that after desquamation there is occasionally articular rheumatism.

As to treatment, I suppose there is a great variety, nevertheless similar to a great extent in principle. As a preventive, I have tried *salol*. In some cases it seems to act perfectly well, while in others it is of no avail. In the early stages when marked symptoms of overpoisoning are manifested, I use potassium chlorate in doses to suit the age of patient every two or three hours. For the throat symptoms I use peroxide of hydrogen every two or three hours. Hot fomentations applied early to the swollen glands of the neck abort abscesses. For the fever, sponging with cold water or the cold pack whenever indicated. In order to be on the safe side, I usually give a mixture of digitalis and sweet spirits of nitre in doses to correspond with the age of the patient. This seems to cleanse the kidneys and thus many a complication may be avoided. This solution I also use in the case of nephritis. To prevent further contagion I order the patient to be anointed with carbolized vaseline, while all the clothes are either burnt or soaked in a suitable germicide solution. When the bowels are constipated calomel acts very nicely.

I may have omitted many points in the treatment, but in 70 cases I have lost but seven, three in the early stage from over-poisoning, two from nephritis, one from post-pharyngeal abscess, another died from some cause which I could not ascertain.

BRONCHITIS MIXTURES.

The following seasonable excerpts are from *Guy's Hospital Gazette:*

℞

Carbonate of ammonium	grains	16.
Syrup of tolu	fluid oz.	½.
Tincture of squill	minims	40.
Compound tinct. of cinchona	fluid drachms	2.
Spirit of chloroform	minims	4.
Rose-water	fluid oz.	2.

Mix. Dose, 1 fluid drachm every four hours.

Dr. J. Davies says of this that it is "stimulating expectorant in bronchitis and pneumonia."

Dr. W. T. Caldwell says that "for chronic bronchitis in old people, dispensed as a solution, preserved with a little spirit, the following is much preferred to taking as a pill. The quinine acts as a germicide, and the purulent discharge is removed."

℞

Sulphate quinine	grain	1.
Extract of hemlock	grains	3 to 5.
Dilute hydrobromic acid	minims	4.
Water to	fluid oz.	1.

Mix. Dose, 1 fluid oz. every four hours.

Another, of which Dr. R. Woods speaks highly, is:

℞

Tartarated antimony	grain	¼.
Spirit of chloroform	minims	12.
Tincture of opium	"	7.
Burnt sugar	grains	8.
Water to	fluid oz.	1.

Mix. Dose, 1 fluid oz. three times a day.

SELECTED FORMULÆ.

CONSUMPTION.

The following (*O Galenos*, Etos I, Arithmos 9) is praised for internal administration in pulmonary tuberculosis:

℞
Creasot. pur.....fl. ʒ j.
Spirit vini.....fl. ʒ iijss.
Vini Santorini.....fl. ʒ x.
Syrup simplicis.....fl. ʒ iijss.
A large spoonful three times a day.

ANTISEPTIC TREATMENT OF PTYRIASIS VERSICOLOR.

The pathogenic rôle of the *microsporon furfur* is secondary, the parasite developing only on the skin of dyspeptics affected with seborrhœa. Hence, there are two indications in the rational treatment of this affection.

General and Antidyspeptic Treatment.—(1) Encourage intestinal evacuations by purgatives, salicylate of magnesia, castor-oil. (2) Associate intestinal antiseptics with these purgatives and with alkalies.

℞
Bicarbonate of soda,
Salol,
Magnesia.....ää grammes 5.
Divide into fifteen powders and give one before each meal.

Local Antiseborrhœic and Parasiticide Treatment.—(1) Each morning hot lotions and frictions with green soap, allowing the foam to dry on, and subsequent cleansing bath with plenty of water. (2) Every second day, sulphur baths followed by frictions with green soap, with the addition of naphthol or pumice-stone.

℞
Green soap.....grammes 500.
Finely pulverized pumice-stone " 250.

(3) Daily antiseptic inunctions with the following mixture:

℞
Resorcin.....grammes 5 to 10.
Ol. ricini..... " 45.
Spt. vini rect..... " 150.
Bals. Peru.....centigr. 0·50.

or,

℞
Acid. salicylic.....grammes 2 to 3.
Sulph. precip..... " 10 to 15.
Lanolin..... " 70.
Vaseline..... " 18.

—*Revue Gén. de Clin. et de Thérap.*, No. 49, 1891.

RHEUMATISM.

The following prescription is a good combination in rheumatism. I have tested it personally, and know whereof I speak:

℞
Pot. iodide.....ʒ iiss.
Tr. cimicifuga.....ʒ iss.
Vin. colch. sem.....ʒ j.
Fl. ext. hentane.....ʒ ss.
Simple syrup.....ʒ v.
M. Sig. Teaspoonful well diluted with water every four hours.

Dr. Conger, in N. E. Med. Monthly.

OTALGIA.

In otalgia the following (*Ugeskrift for Læger*, Nos. 28 and 29, 1891) is recommended:

℞
Chloral camphor.....grammes 5.
Glycerine..... " 30.
Ol. am gdal. dulc..... " 10.
Dip a pledget of cotton into this mixture and introduce it into the meatus.

MONIN'S KOLA ESSENCE STIMULANT.

Essence of kola (roasted).....Gramme 40.
Tincture of canella..... " 5.
Essence of peppermint.....gtt. 10.
Flavor, mucilage.....ad. lib.
Water, enough to make.....Gramme 150
This is to be consumed in 24 hours.

—*Bulletin Medicale.*

EMMENAGOGUE.

The following is recommended as a reliable emmenagogue in many cases of functional amenorrhœa:

℞
Bichloride of mercury,
Arsenite of sodium.....ää gr. iij.
Sulphate of strychnine..... " iss.
Carbonate of potassium,
Sulphate of iron.....ää " xlv.
Mix, and divide into 60 pills. Sig. One pill after each meal.

—*Revue de Médico-Chirurgicale des Maladies des Femmes; Times and Register.*

CHLOROFORM SALVE FOR LOCAL ANÆSTHESIA.

Dr. Kittel (*La Independencia Medica*, No. 42, 1891) uses the following:

℞
Chloroform.....part 1.
Ceræ..... " 1.
Adipis.....parts 3.

APPETIZING TONIC.

℞
Quiniæ sulph.,
Ferri citr.....ää ʒ ss.
M. Ft. capsulæ No. 40. Sig. One after meals

DIARRHŒA OF CHILDREN.

Dr. E. M. Sympson (*La Semaine médical*, No. 59, 1891) recommends the following:

℞
Borax.....grammes 4.
Glycerin. pur..... " 16.
Tint corticis aurant..... " 3.
Aq. destillat..... " 60.
A teaspoonful every one to three hours.

This is not only an excellent anti-diarrhœic, but also quiets the colic.

WHOOPING-COUGH.

Dr. Schmid (*Lo Sperimentale*, No. 21, 1891) uses the following in a spray with success in cases of pertussis:

℞
Acid carbolic.....grammes 0.03.
Sol. menthol (4 per cent.)..... " 20.
Sol. cocain. (3 per cent.)..... " 15.
Aq. laurocerasi..... " 60.
Spray the child's throat once an hour, or even oftener.

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LEADING ARTICLE.

THE INFECTIOUSNESS, CONTAGIOUSNESS, AND HEREDITY OF LEPROSY.

Professor A. von Bergmann contributes to the *Sammlung klinische Vortraege* an article, which is an interesting and valuable exposition of this subject which has of late been brought so forcibly to our attention, both by the press and clinical experience in the larger cities of the United States.

In the city of New York the subject of the contagiousness of leprosy has become of such importance that the authorities have seen fit to place the matter under investigation, and have decided that the bulk of the evidence goes to answer the question in the affirmative. However, in this decision, no discrimination has been made between what is known as tuberculous leprosy and the variety known as anæsthetic leprosy. There are many authorities whose words cannot be disregarded, who look upon leprosy rather as infectious than contagious.

A. von Bergmann, living as he does in a locality rich in lepra material, has had such a vast experience that his views demand attention. He starts out by saying that he has placed together all cases which came under his consideration without any distinction between the tubercular and the nervous varieties, and as an excuse for this he quotes Danielssen, Dehio, and Gerlach, who consider the scales which come away from the skin in this affection to be parts of a specific lepra tissue. The main distinction between lepra nervorum and lepra tuberosa that he makes is the length of the course which the disease runs; the former lasting often more than twenty years, and the latter ranging from four to twelve years. In contradistinction to these views, we have Kaposi, who at the Tenth International Congress maintained from a practical standpoint the non-contagiousness of leprosy.

Von Bergmann asks: How do the anti-contagionists explain the spread of leprosy? And then goes on to say, "In the first place, through inheritance, next through spontaneous generation, poverty, scant food, special

rancid oil, spoiled and raw fish-roe, and especially from fish food. Hutchinson is a most zealous supporter of the fish nutrition theory, and finds disciples in such men as Hillis, Zambaco, Sandreczki, Wyndham and Cottle. Hutchinson reports more than twelve cases that he observed in London, which without exception emanated from leprous regions and who ran the risk of eating fish, especially salt fish. The beginning of the fish theory dates back to Avicenna. Danielssen and Boeck have investigated fish in Norway which had been attributed as a cause of leprosy, and have between them found examples which possess greater and smaller nodes, and they point to an undoubted relation to the lepra tuberosa. Exact investigation proves that we have to deal here with vegetable parasites.

Petersen and S. Moore allege that on the other hand leprosy appears in people who are remote from the sea, and who never even see a fish. We can, therefore, well pass over the fish theory as long as supporters of the same can bring forth no definite data for the support of the truthfulness of their speculation.

Dr. Bergmann likewise disregards the theory that leprosy is due to extreme poverty on account of the lack of sufficient and exact data to support the theory, and goes on to to say that inheritance of leprosy is considered by most of the auto-contagionists as the chief factor to induce the spread of leprosy.

Kaurin, Carter and A. Wernich have worked out the percentage of hereditary leprosy in Norway, Katywar, East India and Japan. Kaurin finds of the cases of leprosy in Norway 30 per cent. are hereditary. Carter likewise in Katywar finds it to be 30 per cent. A. Wernich finds that hereditary leprosy in Japan is 40 per cent. Out of 107 cases reported by English physicians, in the East India service, 69 cases are put down as undoubtedly hereditary. On the other hand, there are Armauer Hausen and Leloir, Bidenkap, Pr. A. Marrow, J. de Arazedo Lima and many others, who consider that hereditary leprosy can seldom be traced. *A priori*, it is easy to see, that the conditions for the hereditary transmission of leprosy are favorable. The *potentia coëundi* appears in lepers first in advanced stages. The testicles of lepers are very frequently the seat of bacilli and the same has been found by Cornil, Neisser, Hansen and Leloir in the seminal duct. Neisser and Leloir have in a single case found bacilli in the coni vasculosi Halleri. Accordingly it appears that the transmission undoubtedly takes place through the medium of the spermatic fluid.

In a book written by Danielssen and Boeck, in a table of 145 cases of lepra tuberosa, it is shown that 127 cases of the disease were due to inheritance, only 43 were in a direct line of transmission. From these 43 direct cases of inheritance 13 are entirely in the first generation, 25 came from grandparents, one comes from the third and four from the fourth generation. In 145 cases of leprosy, in only 13 cases were the parents leprous; this makes scarcely 9 per cent. A table of 68 cases of lepra nervorum, shows about 10 per cent. of cases in which the parents were leprous. Naturally there are doubtful cases as to the heredity and as to the contagiousness of the disease.

The greatest hindrance to an understanding of the contagiousness of leprosy is conclusively offered in the question of the length of the period of incubation. This period is long, from four to five years intervening between the exposure and the outbreak of the disease.

These cases of direct and indirect transmission are but a handful from the rich material which Von Bergmann has collected. He has handled the matter in a thoroughly scientific way, and the bulk of the evidence from his own cases goes to show that leprosy is contagious and hereditary. He has devoted considerable space to the method of infection, quoting inculation experiments upon animals made with blood taken from leprous patients. In every case of inoculation of the lower animals the results have been negative; of course, the factor of the long period of incubation must not be lost sight of here, even as in other specific granulomata the animal often dying long before the disease has had time to manifest itself.

He has very little to say in regard to the bacillus of leprosy as being its cause, and seems to attach but very little importance to it as a diagnostic factor. After placing all his cases in a table he describes them both separately and in groups and closes his article as follows: "The longer and the more critically that I collect the histories of lepers the easier it is to prove that cases are related that at first appeared isolated. However, it may readily be perceived that these cases furnish a practical support for the theory of transmission of leprosy, for in 60 per cent. of the cases contagiousness is undoubtedly established; in others presumably so. Think of all the cases of diphtheria, scarlet fever, small-pox, *et cetera*, that you come upon in your practice as isolated cases, where in spite of every endeavor you are not enabled to establish the source of infection; also consider the experience which we have had in chronic infective diseases. No one today considers it a proof against the transmission of syphilis, if a syphilitic man inoculates neither wife nor children. Why should it be held for a proof of the transmission of leprosy that man in each case must share his leprosy with his wife. Our material can also be brought to support the length of the period of incubation of leprosy. In not a single case does the same fall below three years, the majority of the cases ranging between four and five years. I believe as soon as the long period of incubation is established, the opposition against its transmission will be silenced."

THE DEATH OF DR. D. HAYES AGNEW.

Dr. D. Hayes Agnew, the greatest American surgeon, died on Tuesday, March 22d, and was buried on Friday, March 25th. An obituary notice will appear in a subsequent issue of the REPORTER.

IT is claimed by good authority that two infectious diseases can exist in the same person at the same time.

BOOK REVIEWS.

TREATISE ON GYNÆCOLOGY, MEDICAL AND SURGICAL, BY S. POZZI, M. D., Prof. Agrégé à la Faculté de Médecine, Paris, etc. Translated from the French edition under the supervision of, and with additions by, BROOKS H. WELLS, M. D., Lecturer on Gynæcology at the New York Polyclinic, etc. Vol. I. Large 8vo, pp. xxiii—581 Illustrated with 305 wood engravings, and 6 full-page plates in color. New York: Willliam Wood & Co., 1891.

The task of the reviewer of this work is a most agreable one. Judging the whole work by the first volume, it is the most important addition to gynæcological literature in recent years. The most striking feature of it is, that it is distinctly in touch with current thought; its matter is neither obsolete nor obsolescent, but everywhere represents what is good in modern progressive gynæcology. Another striking feature is that it is French, and that it sets forth the work of the French in diseases of women from the dark ages down to the present time. The work of all nations is duly credited and with a broad, liberal spirit, but as a true Frenchman, the author never loses an opportunity to bring the work of his countrymen into prominence. This fact gives an added value to the work. Because of the long silence of French writers on gynæcology during the time when writers of other nations have been so prolific, we have rather forgotten the good work which France has done in this department of medicine, and it is well to have the facts recalled to our attention.

Chapter I. treats of antisepsis in gynæcology. This is a useful innovation, as bringing prominently to the attention of the reader the importance of the subject. The only criticism upon this very full chapter is that the editor should have added notes on the use of the steam sterilizer for the sterilization of instruments and dressings; and also the use of sterilized water to cover the instruments during operations, instead of the carbolic solutions. These improvements in technique have become general since the chapter was written by the author. Chapter II. treats of anæsthesia, Chapter III. of methods of suture and hæmostasis, Chapter IV. of methods of gynæcological examinations, each being systematically written. The author still clings to the routine use of the sound and to the use of the tents for dilating the cervix, but the dangers of the practice are pointed out.

Chapter V. discusses metritis, under which head is embraced metritis and endometritis, and laceration and erosion of the cervix.

The classification is adopted because of unity in the ætiology of the conditions—all being inflammatory and due to germ infection. To those who have admitted subinvolution, congestion, and hyperplasia from congestion, and especially those who with Thomas and Emmet have combated the theory as to the inflammatory character of metritis, this classification comes as a shock. The author can easily fortify his position with proofs. For example, subinvolution is almost always due to infection in child-bed, and the conditions brought about by long continued congestion are favorable to infection. As to the primary cause of the conditions embraced under the title "Metritis," being infection in all cases, the author certainly goes too far. Early rising from the lying-in bed can cause subinvolution, displacement of the uterus, and subsequent metritis (a late infection?). Here the primary cause is not infection, and it is well known that the large hyperplastic uterus is usually found in poor women who bore babies at short intervals, and who go to work while the uterus is still subinvolved. And these illustrations might be extended, by studying the endometritis accompanying fibriods, etc.

The author is a strong advocate of the curette in chronic endometritis when it is complicated, the curetting to be done under strict asepsis. This method of treatment is coming more and more into favor.

The relations between inflammation of the uterus and the uterine appendages are more definitely stated than in any other systematic treatise.

Chapters viii–xvi treat of uterine fibroids and cancer. Chapters xvii–xix, contain 104 pages, treating of displacements of the uterus in a specially complete and admirable manner. It is to be regretted, however, that an exception must be made in the chapter on prolapse of the genital organs. The relation of the injuries of the pelvic floor to prolapse of the pelvic viscera is scarcely hinted at, and yet these injuries are almost always the direct cause of the prolapse. Apparently the author is not familiar with Emmet's operation on the perineum, as he makes no mention of it.

Chapters xx–xxii are devoted to inversion of the uterus, certain deformities on the cervix and uterus, and to disorders of menstruation. The two points in treatment which will not commend themselves to most American readers are the use of tents to secure dilatation of the cervix, and the use of the syringe in making intra-uterine applications. The dangers of the tent as compared with the steel dilator should prevent its employment. The author uses the uterine syringe only after preliminary dilatation and curetting, under which conditions its use is a very different procedure than when the cervical canal is of normal calibre.

The notes of the editor are valuable, and reflect great credit. Unquestionably he has placed the profession under obligations to him for this admirable translation of Pozzi's great work.

CORRESPONDENCE.

THE CHEMICAL STUDY OF THE ÆTIOLOGY OF SUMMER DIARRHŒA IN CHILDREN.

EDITOR OF THE MEDICAL AND SURGICAL REPORTER:—In your issue Oct. 17th, 1891, page 637, there is an article under the above title, translated from the Italian, and credited to the investigations of Varyas. This is from an article which was read by me before the American Pædiatric Society in June, 1890, and was published in the transactions of that society and in the Archives of Pædiatrics date of Oct. 24th, 1891.

That the article is a translation and condensation of my paper will be evident to any one who will compare the two.

VICTOR C. VAUGHAN,
Ann Arbor, Mich.

FLORENCE REDIVIVUS.

A striking example of the beneficent influence of sanitary reforms on the health of cities is afforded by a study of Florentine death-rates before and after the improvements which have been carried out of late years. The ameliorations have consisted for the most part in the prevention of floods, the suppression of private slaughter-houses, and a better water-supply, together with sundry house and street improvements. These are still in progress, but on comparing the death-rate from 1818 to 1877, equal to 34.6 per 1,000, to that of the years in which unsanitary conditions were being dealt with, we find that the returns fall successively to 32.8, 28.6, and, in 1890, to 2g.3, and this in spite of a severe epidemic of typhoid fever during last year. It is worthy of note that a similar epidemic occurred at Pisa about the same time with a mortality of 3.75 per 1,000, while at Florence it was only 1.279. According to Sir Douglas Galton, under whose superintendence these works have been carried out to a large extent, Florence only needs a more efficient system of drainage to make it the healthiest city, not only of Europe, but of the world.

PERISCOPE.

THERAPEUTICS.

THE GERM THEORY OF DIPHTHERIA FROM A THERAPEUTIC POINT OF VIEW.

According to the *Bulletin Géneral de Thérapeutique*, October 30, 1891, Dr. Jacques, of Marseilles, promulgates the following doctrine: While diphtheria is due to a specific germ from which emanate toxic properties, causing various disorders in their journey through the economy, the germ itself exists only in false membranes, and does not penetrate the blood or organs. The mouth and false membranes contain other micro-organisms beside the specific diphtheritic germ. Gargles are recommended every hour, first of perchloride of iron in solution, followed by washing the throat with warm water containing three parts boracic acid to every hundred parts water; and the alternate hour, a gargle of carbolic solution, 1 to 100, without washing the throat afterward. For very young children powders may take the place of gargles.

PROPHYLAXIS AND TREATMENT OF INFLUENZA.

Cyrus Edson, of the Health Department of the City of New York, publishes a monograph on "*La Grippe* and its Treatment." Three indications are to be fulfilled: (1) means must be taken to assist the system to rid itself of the poison to which the attack is due; (2) pain must be relieved; and, (3) not the least important, depression must be counteracted. The first indication is obtained by means of castor oil or 2 compound rhubarb pills. Three or four 3-grain powders of phenacetin are usually sufficient to relieve headache and muscular pains. Salol, 2½ grains to each dose, may be added to the phenacetin with advantage. He deprecates antipyrin and its congerers, which serve to augment the depression, and recommends instead Hoffman's anodyne, which is diaphoretic, diuretic, and stimulant. To overcome depression during and after the disease, he recommends the free use of tonics. He repeats Professor Laffont's (of Lille) recommendation of coca preparations, those of Mariani being given the preference. During the disease a hot grog, one-third Mariani wine of coca and two-thirds sweetened water, is administered, taken very hot, several times a day, the slight diaphoresis induced being a valuable addition to the tonic.

Edson considers champagne, generous wines, tonic doses of quinine, iron and strychnia also of value. The catarrhal irritation of the air-passages are best allayed by inhalations of compound tincture of benzoin. Chloroform liniment is recommended as a rubefacient; opium and carbonate of ammonia for the cough. The treatment of pneumonic *grippe* is essentially the same as that of uncomplicated pneumonia, the author emphasizing the advisability of preserving the strength of the patients.

TREATMENT OF ACUTE CORYZA.

Capitan, in *Médicine Moderne*, No. 46, 1891, recommends the following powder, which, he says, arrests generally, almost immediately, a commencing coryza, if from the very onset the patient takes pains to snuff up a pinch into each nostril, and draw it in deeply:

℞

Salol	1 gramme
Salicylic acid	20 centigrammes,
Tannin	10 "
Powdered boric acid	4 grammes. M.

Take a pinch every hour for half a day, and then discontinue the use of this snuff, for if it be persevered with it may cause an eczematous eruption on the margins of the nostrils from the action of the phenic acid formed from decomposition of the salol.

Another snuff-powder by the same writer is recommended as being similar in its action to the preceding, but less powerful:

℞

Powder of talc	5 grammes,
Antipyrine	1 "
Pulv. boracic acid	2 "
Salicylic acid	25 centigrammes. M.

This powder may be snuffed up the nostrils without fear of irritation.

M. Tessier, in the *Annales de Médicine*, indicates several formulæ for the treatment of this affection. The following is a mixture for inhalation:

℞

Acid carbolic fort	5 grammes,
Liquor ammon. fort	5 "
Water	15 "
Alcohol	15 " M.

Pour a few drops on blotting paper and inhale for a few seconds. In some cases, chloroform or tincture of camphor may be advantageously substituted for the water in the above.

Dobson, in the *Lancet*, May 31, 1884, advises respiration of camphor coarsely powdered and placed in a jug of boiling water as an effective remedy against coryza. "About one drachm of camphor should be added to half a pint of fluid, and the steam thus impregnated should be inhaled slowly ten minutes every hour, repeating it three or four

times, when the nasal inflammation will be much relieved. The jug containing the water as well as the face of the patient must be surrounded with a paper cone during the period of inhalation.

The following formula is recommended by Dr. Beverley Robinson:

℞ Pulv. fol. belladonnæ..... .....3 i.
Pulv. morph. sulph.....gr. ij.
Pulv. gum acaciæ, ad .....3 ss. M.
Sig. Use with the powder-blower for anterior and posterior nares.

This powder should be blown through the nose both anteriorly and posteriorly so as to coat over the mucous membrane lining the nasal passages very thoroughly. "Its most noticeable action is to diminish the congested condition of the interior of the nose, so as to permit freer passage of the inspired and expired current of air. This it does, doubtless, by contracting the small blood-vessels and lessening the amount of watery fluid which exudes from them into the cellular structure. Besides all three agents, morphia, belladonna and gum are decidedly antiphlogistic in their action upon the inflamed pituitary."

Dr. Morell Mackenzie advises the following snuff, which is to be used from the commencement of the cold, but never longer than twenty-four hours:

℞ Morph. sulph.....12 centigrammes.
Subnitrate of bismuth.....3 gr. 50. M.

To conclude: A person who is conscious of having taken a cold in the head should take his room and resort for relief to some one of the measures of local treatment above given. For internal medication we do not think that much confidence is to be placed in the small doses of atropine or the larger doses of quinine advised by some authorities. If there be a furred tongue and a deranged state of the *primæ viæ*, a saline laxative, a full dose of rhubarb and soda, or even a cholagogue cathartic may be indicated. Hourly doses (one drop) of tincture of aconite may be given, or two or three five-grain doses of acetanilid two hours apart, or two ten-grain doses of phenacetin at the same or a longer interval, if there be considerable fever and headache. It is worse than useless to take any other nourishment than a little hot liquid food till the acute symptoms have somewhat subsided. General diaphoresis will seldom be advisable for a simple coryza. Dover's powder is apt to disturb the stomach and constipate, and can seldom be required. Antimonials, jaborandi and pilocarpine are not to be thought of. A teaspoonful every two hours of aromatic spirits of ammonia can do no harm; or two or three drops of the liquor morph. sulph. along with two and a half grains of ammonium carbonate every hour for six hours, and afterwards every hour and a half. According to Phillips, a few drops of the tincture of euphrasia officinalis (eye-bright) taken at the beginning of the attack of acute coryza, and repeated every two or three hours will often abort it, and this treatment is endorsed by Dr. G. M. Garland.—*Boston Med. Surg. Jour.*

DUBOISIN AS A SEDATIVE AND HYPNOTIC.

Dr. Ostermayer (*Allgemeine Zeitschrift für Psych.*) regards the sulphate of duboisin as superior to hyoscine, in not having the inconveniences of the latter drug. It is chiefly a hypnotic, producing sleep in from twenty to thirty minutes, and is to be given in doses varying from one to three milligrammes, according to the character of the case. It is said to produce no dangerous or disagreeable symptoms, and although continuous use produces tolerance, by leaving it off for a short time the full effect can be obtained.—*Lancet.*

COPAIBA BALSAM AS A DIURETIC.

Following up Obolensky's researches on the diuretic effect of balsam of copaiba (see Supplement, August 8th, 1891, p. 45), M. I. Svetükhin (*Vratch*, No. 35, 1891) has come to the following conclusions: 1. Copaiba balsam undoubtedly has a very powerful diuretic action. 2. It induces an abundant diuresis not only in cases in which the daily quantity of the urine has previously been subnormal, but even where the quantity has been oscillating within normal limits. 3. The remedy is equally reliable in cases of cardiac, hepatic, and renal diseases, as well as in arterio-sclerosis aud serous pleurisy. 4. Under the influence of the drug serous pleuritic effusions are rapidly absorbed. 5. Neither the arterial tension nor the pulse curve shows any alteration. 6. The diuretic effects are usually obtained even on the administration of 5-grain doses thrice daily; 10-grain doses, given three or four times a day, should be regarded as medium ones; 10 or 15-grain doses, given every two hours, as large ones. 7. Neither the small nor the medium doses ever give rise to gastro-intestinal disturbance or renal irritation. If albuminuria is present they never increase the proportion of albumen in the urine. 8. Given by intravenous injection to healthy dogs, copaiba balsam augments the daily quantity of the urine, but has no effect on the blood pressure.—*Brit. Med. Jour.*

MEDICINE.

CATGUT INFECTION IN THE DRY TREATMENT OF WOUNDS.

Brunner, in *Langenbeck's Archives*, Bd. 42, Heft III, replies to the article of P. Klemm, in the same journal, Bd. 41, which was abstracted in the July issue of the *University Medical Magizine*. He says the conclusions of Klemm are in direct opposition to those he himself had submitted in the *Beiträge zur klinische Chirurgie*, Bd. 6, Heft I. Klemm favored the use of silk, and argued against the asepticity of catgut, quoting the remarks of Bazzini and Biondi at the Italian Surgical Congress in his support. The author cites Novaro, Postempski and Bonnano as upholding his views at the same meeting, particularly the latter, and says that when suppuration occurs in the track of a deep catgut suture it soon stops with absorption of the suture, but if silk is used it persists until the thread is removed. Both silk and catgut, when buried in the tissues, produce a local disturbance favoring the development of organisms, but the catgut less than the silk, because it remains there a shorter time. Such infection, when it occurs, he attributes to outside sources and not to the catgut itself. Klemm claimed that his experiments showed that silk sutures were less liable to provoke suppuration than catgut ones. To this Brunner replies that the experiments were too few in number, and recalls the failures of Lister, Socin, Mosetig, Kocher and nine other operators with silk ligatures; also, the report on its use in Billroth's clinic by Von Eiselberg to the same effect. That catgut, as a rule, heals readily in deep wounds has been too often demonstrated by experiments to be overthrown by the four experiment of Klemm. The twenty experiments in which the author buried catgut which had been infected with the germs of splenic fever, and then sterilized, were not noticed by Klemm. Catgut has been used in the Zurich clinic for ten years with good results. Socin gives twenty-three cases of extirpation of goitre in which buried catgut sutures were frequently used with perfect satisfaction; also, Kappeler writes that he, too, has good results in its use. Brunner concludes as follows: Any method of wound treatment which does not preserve the sterile catgut from secondary infection is inefficient, and all the more so if drainage is dispensed with; if lack of proper disinfection of the catgut is such a cause of failure as Klemm holds, then we ought to have them occurring as a rule and not the exception. Catgut will retain its place in surgery along with silk, but it should only be used after being thoroughly soaked in an efficient disinfectant, and not employed when simply sterilized by heat alone.—*Univ. Med. Mag.*

PATHOLOGY OF PROTEIDS.

S. Martin (*Brit. Med. Jour.*, January 1, 1891; *Jour. Chem. Soc.*, 60, 761). The changes in structure that occur in a diseased tissue or organ are associated with chemical changes, and it is to certain changes in the proteids, the most abundant constituent of most living structures, that attention was chiefly directed in the present research; although peptones and albumoses are found in the alimentary canal, their presence elsewhere in the body is pathological, as during normal absorption they are regenerated into the blood proteids. The action of microörganisms during putrefaction is, first, similar to that of pepsin, namely: it forms albumoses and peptones, but, subsequently, ptomaines may make their appearance. Putrefaction (using the word in the wide sense of the changes due to microbes) is, moreover, a frequent factor in pathological processes, and albumoses and peptones are poisonous substances. If these poisons are found in decomposing collections of cells, as in abscesses, empyema, phthisis, etc., they pass into the lymph and blood-streams, passing ultimately from the body by the urine, constituting what is generally termed peptonuria, but which should be more properly called albumosuria, the proteid which is present being deutero-albumose; Illustrative cases are cited which show the amount of albumose in the pus and in the urine before and after the evacuation of the abscess, the albumose in the urine varying with the amount of accumulated pus.

One of the most striking features of pus-formation which is common to it and albumose poisoning is fever. It is possible that other factors have also to be considered; thus leucine, often found in pus, is a fever producer; alkaloids, if they do exist, have yet to be isolated.

In ordinary pus, the agent which forms the albumoses is probably not an euzyme, but the *staphylococcus pyogenes aureus;* in tubercle, it may be the tubercle bacillus.

But the presence of albumoses in disease cannot always be ascribed to microörganisms, as in glycosuria and osteomalacia. In osteomalacia the diseased bones contain an albumose similar to that execreted in the urine, and, perhaps, due to the rapid breaking down of lowly organized cells. In the

puerperal state, the origin of the albumose may be the degenerating cells of the hypertrophied uterus which is undergoing involution. The origin of albumoses in other cases, such as measles, is at present involved in obscurity.

NEURASTHENIA AND VARICOCELE.

Dr. Wiederhold has observed quite a number of cases of neurasthenia which were evidently caused by varicocele, since treatment directed toward the latter disease effected such a marked improvement of the neurasthenia as was not observed to follow the treatment of neurasthenia uncomplicated by varicocele.

The cases of neurasthenia accompanied by varicocele were divided into two groups:

The first group was characterized by decreased mental powers. The patients (mostly young adults not particularly given to excesses) suddenly broke down at a time when their mental powers were taxed a little more than usual. They would sit for hours before an open book without comprehending what they were mechanically reading. Their physical powers were also much impaired, even the slightest exertion causing great fatigue. The sexual organs exhibited signs of irritable weakness. They were almost always excited, while coitus was impeded on account of premature ejaculation or incomplete erection.

Accompanying the above symptoms was great mental depression and despondency, leading at times to thoughts of suicide.

In another group of cases the psychical symptoms were less marked, while symptoms referred to the periphery of the nerves were prominent. Neuralgias, especially of the intercostal nerves, were frequent. Other symptoms were irregular heat, dyspepsia, paræsthesia, deafness, a feeling of heat or cold in particular limbs, increased sweating on the side of the body on which the varicocele was located.

The treatment, directed towards the varicocele which effected such prompt amelioration of the neurasthenia, consisted of electricity, hydrotherapy, and mechanical support by means of a suspensory. Surgical means were altogether omitted as the patients were treated in a hospital for nervous diseases.

The treatment employed is supposed to have relieved the varicocele by restoring the tone of the dilated veins.—*Deut. Med. Woch.*

Have sick room well lighted—sun light.

SURGERY.

COCAINE ANÆSTHESIA FOR ENUCLEATION OF THE EYE.

Dr. Edward Jackson, in an article in the *Annals of Ophthalmology*, says: While cocainization has replaced general anæsthesia for the great majority of the operations done in opththalmic practice, the limits of its usefulness in that indicated by the heading of this paper are not entirely settled. Probably most surgeons regard enucleation of the eye as always calling for the use of a general anæsthetic, but a few report that they do the operation with the aid of cocaine anæsthesia alone.

The writer's experience in this direction began with the attempted enucleation of an eye long blind, containing a foreign body, and which had become violently inflamed a few weeks before, and had caused sympathetic irritation in the other eye. A four per cent. solution of cocaine of proved efficiency was freely instilled in the conjunctival sac, and then injected into the orbital tissues with a hypodermic syringe with a freedom that would now be considered dangerous. The patient was a strong young laboring man, and the operation carried forward as rapidly as possible; but the evidently intense suffering of the patient led to the completion of the enucleation under chloroform.

This was in the early weeks of our acquaintance with cocaine as an anæsthetic, and the one experience led the writer not to make a second attempt for some years. As we became more familiar with the effects of the drug, however, it was noticed that its power of producing insensibility was very slight when the tissues to which it was applied were actively hyperæmic or inflamed; and the idea occurred to the writer that probably this was the explanation of the failure in the above case. Its very satisfactory influence for tenotomy or advanced operations made it seem probable that with the tissues in a similar condition of freedom from hyperæmia it should answer for the enucleation of the globe. A patient requiring the enucleation of a globe quite free from hyperæmia, and desirous of avoiding the use of ether, offered the opportunity for another trial. In this case the result was perfectly satisfactory, there being no complaint or evidence of severe pain. Certainly the pain was less than in the average operation for advancement of tendon.

From that time the writer has used cocaine for the enucleation of all eyes free from

hyperæmia, and has encountered but little more difficulty and caused but little more suffering than attends an ordinary squint operation. For the enucleation of hyperæmic eyes, which are a majority of those requiring enucleation, the use of ether is still resorted to.

For enucleation, cocaine is to used very much the same as for an advancement, but more freely. A four per cent. solution is instilled freely into the conjunctival sac at intervals of two or three minutes, for fifteen or twenty minutes before the operation is commenced. During the operation the cut tissues may also be bathed with the solution, but this part of its use is comparatively unimportant. No attempt is made to inject it in the deeper tissues. The most painful part of the operation is, as in squint operations, the dragging on the tendons of the recti muscles. And the total pain caused will depend mainly on the gentleness or rudeness exhibited in this portion of the operation.

INSTILLATIONS OF NITRATE OF SILVER IN THE TREATMENT OF EPIDIDYMITIS.

Dr. Samuel Alexander calls attention to the value of instillation of nitrate of silver in the posterior urethra to prevent recurrence of the epididymitis, and to hasten the course of the original disease. He does not claim originality in this method which he ascribes to Diday, but insists on its beneficial effects. His plan is to commence as soon as possible with the instillations. The patient is placed in bed, given a subcutaneous injection of morphine if the symptoms are very acute, and then with Keyes' modification of the Ultzman syringe an injection in given just within the membranous urethra. The strength of the solution should vary from 1 gr. to the ounce, if the symptoms are very acute, to 8 gr. to the ounce, and the injection must be repeated at intervals of 24 to 48 hours. During this treatment the testicle should be supported and poulticed with flaxseed. Later, the dry dressing of cotton-wool, vaseline, and rubber tissue covering should be employed. The injection should be made soon after the patient has passed his water, so that there may be as little secretion as possible in the urethra. The advantages as summed up by Dr. Alexander are: (1) The lessened liability to relapse. (2) The shorter course of the disease. (3) The shortening of the duration of the accompanying urethritis, because treatment of the posterior urethra is continued during the existence of the epididymitis.—*Journal Cutaneous and Genito-Urinary Diseases*, December, 1891.

EPISTAXIS: AN EASY AND EFFECTUAL METHOD OF PLUGGING.

Dr. A. A. Philip writes in the *Lancet*: Uudoubtedly plugging the nares by aid of Bellocq's cannula is an excellent method; but occasionally, especially in country practice, Bellocq's cannula is not at hand, and some method easy, effectual, and effected by material alwasy within reach, must be resorted to. Such a method I have found in the following: A piece of old, soft, thin cotton or silk, or oiled silk, about six inches square (a piece of an old handkerchief will answer) is taken, and, by means of a probe, metal thermometer case, or pen-holder, or anything handy, is pushed centre first, "umbrella" fashion, into the nostril, the direction of pressure when the patient is sitting erect being backwards and slightly downwards. It is pushed on in this fashion until it is felt that the point of the "umbrella" is well into the cavity of the naso-pharynx. The thermometer case or probe, or whatever has been employed, is now pushed on in an upward direction and then towards the sides, so as to pull more of the "umbrella" into the naso-pharynx. The thermometer case is now withdrawn. We have now a sac lying in the nares, its closed end protruding well into the pharynx behind, and its open end protruding at the anterior opening of the nares. If it be thought necessary, and is convenient, the inside of the sac may be brushed with some household astringents, such as alum solution, turpentine, etc. A considerable quantity of cotton-wool is now, by means of the thermometer case, pushed well back to the bottom of the sac. Then the thermometer case being held firmly against the packed wool, the mouth of the sac is pulled upon, and thus its bottom with the wool packed in it is pulled forward, and forms a firm, hard plug wedged into the posterior nares. We may now pack the sac full of cotton-wool, dry or soaked in some astringent solution. The mouth of the sac may now be closed by tying it just outside the nostril with a piece of strong thread; it is then trimmed by scissors and the ends of the thread secured outside.

The above method is easier than any I know when both nostrils have to be plugged. It might be suggested to oil the cotton or silk in order to render its introduction easy and prevent it adhering to the mucous membrane, and to render it easy of removal; but I have never found any difficulty without the oil, as the blood renders the material wet and easy of introduction, while the oil does not facilitate removal, and may modify the effect of the astringents that may be used. The

plug may remain *in situ* as long as any other nose plug. In removing the plug, open the mouth of the sac, and with small dressing forceps remove the cotton-wool bit by bit; if there is bleeding simply syringe the sac with weak carbolic lotion or Condy's fluid and repack with clean cotton-wool, or wool impregnated with some antiseptic. If there is no bleeding when the wool is picked out, gently pull out the sac; or if it be adhering to the mucous membrane, syringe in a little warm water, and it may then easily be removed. This method has many advantages. (*a*) It is easy, quickly accomplished, and effectual, and the materials are to be found in every house, and, indeed, about everybody's person (I have plugged in this manner, simply using a handkerchief, one part of which I used for the sac, and the other, torn into narrow strips, in place of the cotton-wool); (*b*) no damage is done to the floor of the nose or back of the soft palate by strings, etc.; (*c*) no disagreeable hawking, coughing or vomiting takes place while the plug is introduced; (*d*) there are no disagreeable strings left hanging down the the throat, causing coughing or sickness while the plug is in; (*e*) the plug can be removed gently without any force, so that no damage is done to the mucous membrane and no return of hæmorrhage caused. I employed this method frequently when in country practice, and do so now in bleeding after operation on the nares, and have always found it to be satisfactory. As the method has been of great use to me, and as I am not aware that any one has spoken of it before, I take this opportunity of mentioning it, in the hope that it may be of use to some brother practitioner when confronted by an urgent case of epistaxis, and other means of plugging are not at hand.

Belfast.

THYROIDECTOMY IN EXOPHTHALMIC GOITRE.

Caird (*Edinburgh Medical Journal*, September, 1891), has reported the case of a man, twenty-five years old, with a large cystic goitre, palpitation, dyspnœa, dysphagia, protrusion of the eyeballs, and increased frequency of pulse, in which, a year after excision of the large cyst and the light lobe of the thyroid gland, the patient felt perfectly well, the eye no longer protruded, the pulse was less frequent than formerly, and there was no discomfort or dyspnœa.

CHANCHROIDS.

℞ Ferri et potass. tart.....drams 2.
Aq. dest.....ounces 4.
M. Sig. Apply as wash.

OBSTETRICS.

PUERPERAL TETANUS.

The indications in the treatment of puerperal tetanus are threefold: (1) Local treatment for the elimination and destruction of pathogenic agents. (2) Modification of the blood which is affected by the toxine. (3) Diminution of the excito-motor power of the nervous centres. We cannot hope much from local antiseptic treatment after the appearance of the first symptoms, but this malady is unusually severe when complicated by septicemia, and in combating the latter we may perhaps diminish the intensity of the principal affection. The greatest caution is necessary in order to avoid violent dilatation of the cervix, which might induce reflex action, and thus increase the muscular spasm. Attempts hitherto made to modify the condition of the blood have been without result, but it is known that immunity from tetanus may be secured in rabbits by injecting into the blood tetanic cultures filtered and raised to a temperature of 60° C., according to Vallard's method, or by introducing terchloride of iodine under the skin, after the example of Behring and Kitasato; and it is claimed by these last observers that tetanus may be cured by the injection of blood serum obtained from animals rendered insusceptible to this disease. The single attempt made to verify this theory by Baginsky resulted unfavorably, and further experiments are rendered difficult by the large quantity of blood serum required. Opium and morphine, belladonna and atropine, chloral and chloroform, bromide of potassium, curara, nitrite of amyl, tobacco, veratrum viride, etc., have all been used in the hope of diminishing contraction and lessening the excito-motor power of the nervous centres. The rare cures reported have been achieved by varied medication, leeches, musk, valerian, warm baths, injections of turpentine, and chloral, but chloroform by inhalation has seemed most frequently beneficial, though the greatest care and skill are necessary in its administration, as the danger of asphyxia in tetanus is constant, and may be increased by chloroform itself. The inhalations must be continued for several hours in order to produce profound and prolonged narcosis. Chloral is next to chloroform in value, and one grain every hour, 10, 12, 14 grains a day may be given by the mouth if possible, or, if the trismus be too great, it may be given in 4-grain doses by enema. If the injections are not retained we may have recourse to morphine, which acts promptly, lessens pain, and induces sleep.

Prolonged warm baths, followed by wet compresses, have a sedative action, and favor elimination of the poison by the skin; but their use must be regulated by the condition of the patient, and they must be instantly given up if they seem to increase contraction. The hygienic surroundings of the patient must be carefully watched, and she must be preserved from physical and mental agitation. The body must be kept at a mild and uniform temperature, and intestinal accumulations removed by enema, though all efforts to rise must be strictly forbidden. Thirst should be assuaged, and the diet made as nourishing as possible, without attempting to open the jaws by force. Prolonged confinement to bed is advisable during convalescence, in order to avoid relapse.—*Gazette des Hôpitaux*, December 3, 1891.

PREVENTION OF MAMMARY ABSCESS.

Tarnier (*Jour. des Sages-Femmes*, December 16th, 1891), insists upon women being kept very clean in childbed. Their hands must be washed, else the nipple may be contaminated by a finger that has just touched the vulva. The nipple should be washed both before and after every act of suckling with water that has been boiled, or with a solution of boric acid. The washing must be done with clean lint or sterilized wool, and not with a sponge. The preliminary washing is necessary, as there might be microbes on the nipple, and the child's mouth might transfer them to a minute fissure, frequent on the nipple after delivery. The second washing removes from the nipple, all milk which, if left there, might become a breeding-ground for germs, and thus set up abscess, or infect the child's mouth and cause aphthæ. Professor Tarnier takes a stronger precaution in his wards. Every woman has her breasts dressed, as a preventive measure, with a compress soaked in a 1 in 5,000 sublimate solution, held in place by strapping, as the pad is likely to slip; a band of gauze is passed round the thorax and wound around the breasts, so as to envelop them completely. There were, in consequence, only two cases of abscess of the breast from November 1st, 1889, to July 15th, 1890, and in both the mammæ were, for different reasons, neglected. This application of sublimate cannot possibly hurt the fœtus. A syphilitic infant can safely take five minims of Van Swieten's solution. The equivalent of that dose in a 1 in 5,000 sublimate solution would be 25 drops, and a child suckling a breast dressed as above described could hardly swallow a trace of the mercury.—*Brit. Med. Jour.*

GYNÆCOLOGY.

GALVANISM IN PRURITUS VULVÆ.

S. S. Kholmogoroff (*Med. Obozrenië*, No. 13, 1891) has tried galvanism in an extremely severe and obstinate case of pruritis with very satisfactory results. The patient, a generally healthy and well-nourished, regularly-menstruating lady, aged 36, married twelve years, had begun to suffer from the complaint, without any assignable cause, two years previously. For the first six months or so the symptoms had appeared only a few days before each menstruation, increasing during the period, and ceasing altogether during the intercatamenial time. Later on it had become incessant, notwithstanding persevering treatment. Nothing abnormal could be seen about the internal or external genitals. The positive electrode—in the shape of a metallic cylinder insulated up to its distal tip by hard rubber—was introduced into the vagina for four or five centimetres, while the cathode—four centimetres in diameter, covered with chamois leather, and moistened with a salt solution—was placed upon the external genitals, and gradually moved over the whole of the itching area. The strength of the current varied from ten to sixteen milliampères, the duration of the sittings from ten to fifteen minutes. After six sittings the disease was completely and permanently cured.—*Brit. Med. Jour.*

AMENORRHŒA OF SCHOOL-GIRLS.

Dr. T. A. Reamy (*Cincinnati Lancet Clinic*), in discussing the amenorrhœa in anæmia, common to school-girls, says: (1) She must leave school, and must not even study at home. (2) She must spend several hours each day in the open air, either walking or riding. In winter she must of course be warmly clad, but must wear no sheepskins or other chest protecting pads. Standing in the open air, she must be induced to breathe deeply with the mouth closed; this should be done for at least fifteen or twenty minutes, and be repeated at least twice a day. Nothing that can be done will more rapidly improve the character of her blood. (3) She must sponge her extremities and body each morning on arising from bed. The water must be of the temperature of the room, and she must practise friction freely with an ordinary towel. (4) She must drink plenty of milk and eat plenty of beefsteak. (5) She must take small doses of iron, combined with some bitter tonic, three times a day. Improvement may be somewhat slow, but if

this course is faithfully carried out, a perfect cure will result, and her education may then be finished. If this course or its equivalent be not followed, the cases will go from bad to worse, and finally die from pulmonary tuberculosis.

DIFFICULTY OF LITHOTRITY IN WOMEN.

Guyon (*Ann de Gynéc.*, Oct., 1891) observes that lithotrity is far harder in women than in men. The introduction of the lithotrite is easy, but the female bladder is either too readily distended or much contracted through pain. In women there is no pouch in the lower and posterior part of the bladder as in man, so that a stone may lie in any part of the vesical cavity. In woman a pouch must be made by pressing the angle of the lithotrite downwards, so that the calculus slips down and may readily be seized and crushed. This manœuvre is easy when the bladder is healthy. When cystitis exists it is very difficult and the calculus must first be seized, then brought into the lower part of the bladder and crushed. In some cases, the stone keeps persistently in the uppermost part of the bladder and has to be crushed there. This condition seems associated with a tendency in the walls of the bladder to fall in and close like the leaves of a portfolio. The results of lithotrity are much the same in both sexes. Guyon has repeatedly seen recovery within a few days, without fever, in women after lithotrity.—*Brit. Med. Jour.*

PROLAPSE OF THE FEMALE URETHRA.

Kleinwächter says (*Zeitschrift für Geburt. und Gynäkol.*, Band. xxii., Hft. 1, 1891) the affection has always been considered a rare one, but the author, after observing two cases, investigated the literature of the subject and was able to collect about one hundred cases. This would go to show that the affection is not so very rare, but that no particular attention has been paid to it; and this is in great part due to the fact that most of the cases occur in children. The prolapse may be as large as a bean, a walnut, or even a pigeon's egg; in fact, Lawson Tait and Baehr observed cases as large as a hen's egg. The prolapsed tissue is hyperæmic, œdematous; pale-red, dark-red, or bluish-red in color; is eroded, suppurating; slightly or very painful, whether coming on suddenly or gradually, etc. The prolapse is seldom a partial one, but is usually complete. Of thirty-four cases it was complete in twenty-four and partial in ten.

PÆDIATRICS

INTUBATION IN RUSSIA.

Jakubowski's results are as follows:

1. Primary diphtheria of the larynx. In fifty-nine cases, intubation was performed in thirty-two, with twenty-one recoveries—65.6 per cent.

Tracheotomy was performed in twenty-seven cases, with two recoveries—7.4 per cent.

In five cases of secondary diphtheria, where intubation was performed, there resulted three recoveries—60 per cent.

From these results the author draws the following conclusions:

Intubation is an operation less dangerous and less difficult than tracheotomy.

2. The inconveniences of intubation are: (*a*) the patient should be under the constant charge of the physician, and (*b*) the introduction and removal of the tube provokes disagreeable symptoms.

3. Secondary pneumonias follow more often intubation than tracheotomy.

4. In croup limited to the larynx and to the superior part of the trachea intubation is the procedure necessary.

5. In a diffuse croupous inflammation of the larynx, trachea, and bronchial tubes, tracheotomy is the operation required.—*Bull. Gen. de Thèrapeutique*, December 15, 1891.

GASTRO-INTESTINAL HÆMORRHAGE IN THE NEW-BORN.

Two cases of this character are reported (*Rev. Mens. des Mal. de l'Enf.*, October, 1891), and there is also a study of certain accidental hæmorrhage with local causation. They occur in children who are usually normal in appearance, they have little or no tendency to recurrence, and may be followed by complete cure. The quantity of blood lost varies; there may be only a single discharge, or there may be several, the quantity lost being sufficient to cause death from acute anæmia, or from the weakness which follows the accident. Hæmatemesis alone rarely occurs. There is most frequently a discharge by the mouth and rectum, or by the rectum alone. In one of the author's cases, intestinal hæmorrhage occurred the day after birth, and in the other, hæmatemesis, which was followed on the next day by a large intestinal hæmorrhage. Both children recovered. These cases followed the rule that such hæmorrhages usually occur within thirty-six hours from birth. Such bleedings are probably due to new conditions of the

circulation in the walls of the stomach and intestine, the circulation being more active than during fœtal life. For treatment the author recommends a warm bath at 38° C. This was efficient in his case in which the hæmorrhage recurred.

COMPLICATIONS OF CHICKEN-POX.

Demme (*Wein med. Blätter*, Nos. 3 and 4, 1892), discusses the complications of varicella observed by him in Berne during 1890. He relates two cases in which there were very marked prodromata; in one case there was for three days high temperature, somnolence, anorexia, nausea, and, finally, a convulsive seizure; in the other, there was severe blood-stained diarrhœa, accompanied by vomiting and fever. After these symptoms had lasted eighteen hours, a copious eruption of varicella appeared, and the symptoms disappeared so quickly that in two hours and a half the temperature was normal, and in twelve hours more the child was practically convalescent. He met with two cases in which the eruptive period was unusually prolonged; in both the successive crops of pocks which appeared at intervals of three or four days were accompanied by a scarlatiniform erythema, and in both nephritis occurred as sequela. He met with two cases in which the rash became gangrenous: one, a boy, aged 2¾ years, died; the other, also a boy, aged 2 years and 2 months, recovered. The treatment in the successful case consisted of careful feeding and the application of tincture of iodine to the gangrenous spots, previously cleaned with the following modification of Lugol's solution: pure iodine 1 part, iodide of potassium 10 parts, distilled water 500 parts; finally, the part was packed with iodoform gauze. The bacteriological examination of these cases did not yield trustworthy results, and nothing was observed in either case to throw any light on the cause of the occurrence of gangrene. Demme also met with two examples of ulceration of the varicellar pocks, an event which he considers to be rare. In one case there seemed to be little room to doubt that the ulcerative process was tuberculous, the infection having taken place through the mother, who was in an advanced stage of pulmonary phthisis. The treatment adopted in this case, which was successful, consisted in a daily warm bath containing about 7½ grains of corrosive sublimate, painting of the ulcers with iodoform dissolved in ether and subsequent packing with iodoform gauze, and the internal administration of syrup of the iodide of iron.—*Brit. Med. Jour.*

HYGIENE.

BACTERIOLOGICAL EXAMINATION OF WATER.

The chief interest attached to the bacteriological examination of water, writes Professor P. A. Frankland in *Nature*, lies in its application to the hygiene of water-supply, inasmuch as it is all but certain that two at least of the most fatal zymotic diseases—cholera and typhoid—can be, and are constantly propagated through the presence of specific micro-organisms in water, and indeed the majority of the bacteriologists are agreed as to the particular forms responsible for these diseases. On this account it is conceived by many that the primary object of the bacteriological examination should be the search for such pathogenic microbes. It is obvious that if the typhoid organism could be detected with unerring certainty in any water in which it was present, a search for this bacillus in the ordinary course of water examination would still have only a very subsidiary interest. Waters are surely not only to be condemned for drinking-purposes when they contain the germs of zymotic disease at the time of analysis, but in all cases when they are subject to contaminations which may at any time contain such germs. Sewage-contaminated waters must on this account be invariably proscribed, quite irrespectively of whether the sewage is, at the time that the water is submitted to examination, derived from healthy or diseased persons. In the present state of our knowledge there can be no doubt that chemical analysis affords us in general a better, although a far from perfect, indication of sewage contamination than do the results of bacteriological examination. The real value of these bacteriological investigations, if judiciously applied, consists in their power of furnishing us with information as to the probable fate of dangerous organisms, should they gain access to drinking-water. It is by their means that we have learnt that many such organisms can preserve their vitality—nay, in some cases can actually undergo multiplication—in ordinary drinking-water; that they are destroyed by maintaining the water at the boiling point for a short time; and that they are more or less perfectly removed by some processes of filtration and precipitation, whilst other processes of the same nature are worthless, or even worse.

Strychnine is used as an antidote for snake bite in Australia.

HYOSCYAMINE IN LETTUCE.

Although preparations of lettuce have from very early times had a reputation in medicine for their soporific properties, the narcotic constituent of the plant has never been ascertained with any certainty. Various neutral, fatty, and waxy bodies separated from the milky sap of different species of Lactuca have been from time to time described as compounds of medicinal value, but on the other hand it has been denied that the dried milk-sap, lactucarium, in spite of its narcotic odor, possesses any sedative action, and in fact this preparation is no longer officinal in England or the United States. It is therefore interesting to learn in a communication from the Research Laboratory of the Pharmaceutical Society, read last week before the Clinical Society, that Mr. T. S. Dymond has established beyond doubt the presence of hyoscyamine, the principal alkaloid of belladonna and henbane, not only in the cabbage and Cos varieties of the common lettuce, L. sativa, but also in the wild lettuce, L. virosa. The amount in the young plants is certainly very minute, but in the official green extract, which according to the directions of the *British Pharmacopœia* is to be prepared from the flowering herb of L. virosa, the mydriatic alkaloid occurs to the extent of 0.02 per cent. As the physiological action of the drug closely corresponds with that of hyoscyamine, there is little doubt that the sedative properties of the lettuce are due to this alkaloid; and though the quantity present in the plant during the early stages of its growth is not sufficient to restrict its consumption as a salad, the observation suggests some explanation of the serious and even fatal results recorded in cases of immoderate consumption of lettuce. It is also interesting to find an alkaloid, which has previously only been associated with the natural order Solanaceæ, as a constant constituent of a member of the Composite group, in which alkaloids have been comparatively rarely observed.—*Brit. Med. Jour.*

DIET IN FEVER.

The question of diet in fever is a most important one, and demands careful attention at the hands of the practitioner. The various metabolic changes associated with a high or raised bodily temperature, and the physiological effects which result therefrom naturally indicate that proper or improper dieting during the febrile state may be all powerful for good or emphatically harmful. A French contemporary, in discussion a new work by Mauquat, draws attention to some practical facts in connection with this subject, to which some reference may here be made. It is generally admitted that in the febrile state gastric digestion is at a low ebb, owing to the diminished secretion of gastric juice; moreover, there is a sensible deduction in one of its constituents, namely, hydrochloric acid. It is for this reasons that patients suffering from fever find acid drinks so grateful to their palates. The proportion of pepsin is not reduced, but both the pancreatic and biliary secretions are much diminished, rendering the digestion of fatty matters almost impossible. It is pointed out that eggs are very unsuitable for food in the febrile state, their digestion requiring a large amount of hydrochloric acid, which is always deficient in these cases. Meat, of course, falls into the same category. With regard to milk, its chief utility seems to be in the water and salts which it contains. If a little hydrochloric acid be added to it, its digestion would be almost assured. Farinaceous materials are well digested by fever patient, and for this reason may be administered in suitable cases. Beef tea as ordinarily made does not contain much albumen, but it is rich in salts, and is useful on these grounds. To generalize, however, beyond a certain point in regard to diet in fever would fail to be practically serviceable; each case, therefore, of fever must be treated, from a dietetic point of view, on its own merits, and the practitioner would do well to bear in mind that he has to steer a middle course course between overfeeding and under-feeding. A too low diet is obviously disadvantageous to the patient, while the other extreme is just as harmful.—*Med. Press.*

EYE LOTIONS OF BORIC ACID FOR INFLUENZA.

Dr. Richard Sisley, in a paper read before the International Congress of Hygiene and Demography in London last summer, quoted Dr. Bezly Thorne, who recommended that the eyes of people exposed to infection should be bathed with a solution of boric acid, and found that none of those who adopted that practice suffered from the disease. The recommendation was founded on the theory that the poison of influenza is absorbed by the conjunctiva.

FOR INCONTINENCE OF URINE.

℞ Tinct. nucis vomicæ.....f ʒ vj.
Ext. damianæ fl.....f ʒ iij.
Glycerini.....q. s. ad f ʒ iv.
M. Sig. ʒ j three times a day, after meals, in a wineglassful of water.

MEDICAL CHEMISTRY.

EFFECT OF HEAT ON CHLORALAMIDE.

It is a common practice in making aqueous solutions of chloralamide to hasten the process by the aid of heat. It is pointed out (*Therap. Monatsh.*) that this drug decomposes when its temperature is raised to over 60° C. Hence the solution should always be prepared in the cold way.

ARSENICAL COD-LIVER OIL.

Arsenical cod-liver oil, upon the request of a specialist in children's diseases, was prepared as follows: 0.5 gm. arsenious oxide was warmed with 20 grams absolute alcohol in a small flask; no solution took place until a small particle of potassium carbonate was added, when the oxide immediately dissolved without dissolving the potassium carbonate; after filtering, the solution was added to 1,500 grams cod-liver oil, and warmed on a water-bath until the alcohol was dissipated. The oil is perfectly transparent, and holds arsenious oxide in solution; 30 grams of the preparation contain 5 mg. arsenious oxide [this is not correct if the arsenious oxide be completely dissolved; 30 grams will then contain 10 mg.—F. X. M.] and can be given to children in doses of ½—1 teaspoonful.—A. Janssen, *Pharm. Ztg.*, 1891, 780.

SOLUTOL AND SOLVEOL, TWO NEW DISINFECTANTS.

The firm of "Dr. F. von Heyden Nachfolger" in Radebeul, near Dresden, the original manufacturers of salicylic acid under Kolbe's now expired patent, have put on the market a soluble form of cresol under the name *solutol*. The solubility of cresol is brought about by the pressure of cresol-sodium. The preparation contains, in 100 cc., 60.4 gr. of cresol, one-fourth of it in a free state, and the remainder as cresol-sodium.

Solutol has been found to be a most efficient disinfectant for general domestic or hospital purposes, such as sinks and water-closets, infected clothing, sputa of consumptives, etc., and an excellent preservative of dead bodies.

It is suitable for medical or surgical disinfection, owing to its strongly alkaline character.

Solveol, on the other hand, is a disinfectant and antiseptic specially designed for surgical purposes. In this the solution of cresol is brought about by the presence of sodium cresotate. The compound is soluble in water to a perfectly clear liquid, which, while much more efficient than carbolic acid, is at the same time much less poisonous.

For medical or surgical purposes the most suitable strength of solution is:

Solveol	27 cc.
Water	to make 2,000 "

For disinfecting the air in sick-rooms, by spraying, 37 cc. of solveol should be dissolved in 480 cc. of water.—After *Zeitschr. d. Oest. Ap. Ver.*, December 10th.

THE ALKALOIDAL VARIABILITY OF OPIUM.

Nearly all the published criticism regarding opium chiefly refers to the percentage of morphine, little attention being paid to the amount of narcotine present. We are, therefore, pleased to notice that M. Adrian has published (*Nouv. Remed.*) the results of thirty-eight assays made at his pharmaceutical factory during the past few years. With two exceptions all the samples showed above seven per cent. of morphine, the average being ten per cent., while the average narcotine content was 2.5 per cent. The average in the latter case gives little idea of the variation actually present, so we quote a few of the figures:

Morphine	Narcotine	Morphine	Narcotine
10.21	2.947	9.00	0.50
11.00	3.343	12.065	3.05
8.066	1.833	8.50	1.90
10.515	3.85	8.48	3.49
8.35	.52	9.203	3.906
9.208	1.555	7.51	.76
12.15	.25	6.75	2.30
11.666	2.025	3.875	3.45

The method of assay is not mentioned, but the want of that information does not detract from the value of these figures so far as comparison is concerned, and they suffice to explain how some preparations cause little or no headache, while others give intense dissatisfaction.

FATAL HÆMORRHAGE FROM SCARIFICATION OF THE CONJUNCTIVA.

Shirley (*New York Medical Journal*, 1892, lv, 1, p. 15) has recorded the case of a mulatto infant, six weeks old, with hereditary syphilis, in which fatal hæmorrhage followed scarification of the palpebral conjunctiva for purulent conjunctivitis. Hot water, ice, and styptics failing to restrain the hæmorrhage, sutures were passed beneath the bleeding surfaces and the upper and lower lids were united. The hæmorrhage was controlled, but the child died.

NEWS AND MISCELLANY.

CHIROGRAPHY AND MERIT.

It is related of the late Sir Richard Quain, whose handwriting was execrable, that when a student he once went up for an examination, in which he confidently expected to take the prize. However, the medal was given to somebody else, and on inquiry it was found that his writing was so bad that the examiner had been able to read only one of the answers.

MORPHINOMANIACS AND HYPODERMIC SYRINGES.

Owing to the recent publication of a case of death after injection of a small amount of cocaine into the tunica vaginalis, there have been numerous articles in the French medical papers on the entire question of injections of powerful solutions. M. Verneuil has shown that the much-vaunted hypodermic injections of antipyrin have proved of more harm than good; They often fail to cure neuralgia, lumbago, etc., and always cause pain, not infrequently complicated with wide ecchymoses, œdema, lymphangitis, and neuritis. These dangers may be avoided if only weak solutions and clean syringes be used, but there is no doubt that there can be but one opinion on the use of unprescribed hypodermic injections by the general public. Last summer, when traveling in a railway carriage in the South of France, an English medical man heard a middle-aged lady proudly boast that she had a *névrose* which could only be assuaged by frequent hypodermic injections of morphine. She then raised her skirt so as to expose the bare skin of the thigh immediately above the stocking, and gravely injected a syringeful of morphine, giving her fellow-passengers, who listened with intense interest, a long account of the subjective symptoms that would follow. The *Gazette Hebdomadaire de Médecine* asks if legislation is not needed to forbid druggists and instrument makers from selling dangerous solutions and hypodermic syringes direct to persons who employ morphine, etc., without medical advice.—*Brit. Med. Jour.*

INTERNATIONAL PERIODICAL CONGRESS OF GYNÆCOLOGY AND OBSTETRICS.

The Belgian Society of Gynæcology and Obstetrics, under the patronage of the Belgian Government, has taken the initiative in organizing "The International Periodical Congress of Gynæcology and Obstetrics," the first session of which will be held in Brussels, September 14 to 19 inclusive, 1892. Three leading questions will be offered for discussion:

1st. Pelvic Suppurations; Referee, Dr. Paul Segond, Paris.

2d. Extra Uterine Pregnancy; Referee, Dr. A Martin, Berlin.

3d. Placenta Prævia; Referee, Dr. Berry Hart, Edinburg.

Fees: Members participating in the first session, 30 francs. (This will entitle the holder to a copy of the proceedings of the Congress.)

Founders (Life Membership), 300 francs.

In the connection with the Congress there will be an International Exposition of instruments and appliances, pertaining to Gynæcology and Obstetrics.

All communications pertaining to this Congress should be mailed direct to the American Secretary, who will promptly furnish all information. All notifications to be forwarded should be received by August 1st.

Everything points to a great success in this Congress. Though notices concerning it have been rather late in this country, already men of celebrity have promised to visit and contribute papers. Among the many foreigners who have written to the Secretary General endorsing and promising support to the undertaking, may be mentioned the following eminent men:

BELGIUM.—De Roubaix, Sacre, Mirriar, Pigeolot, Charles, Sanpart, and others.

ITALY.—Porro, La Torre, Mangiazalli, Bozzi, Morisain.

TURKEY.—Chatazian.

FRANCE.—Pean, Demous, Fochier, Auvard, Doleris, Pozzi, Tarnier, Budin, Terrillon, Terrier, and others.

HOLLAND.—Stokvis, Treub, Nyhoff.

ENGLAND.—Lawson Tait, Wm. Priestly, Champneys, F. Elder, J. White, Watt Black, Thornton, Doran, Specer Wells, Bantock, and others.

AUSTRIA.—Pawlik, Albert, Chrobuk.

GERMANY.—Martin, Leopold, Sänger, Gusserow, Veit, Winckel, Hegar, Kaltenbach, Freund, Heyder, and others.

FINLAND.—Engstrom, Heinricius, Pippingohold.

SWITZERLAND.—Reverdin, Vuillet.

RUSSIA.—Slaviansky.

SWEDEN.—Salisa, Westernark,

NORWAY.—Statfeldt, Howitz, Meyer.

Further details will be furnished as soon as received.

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